MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. LXI. JUNE 14, 1901. No. 8

E. B. KNOBEL, Esq., Vice-President, in the Chair.

Francis William Crook, B.A., Barrister-at-Law, 4 Overcliff, Gravesend, Kent;
Frank Lowman, B.A., Lecturer in Science, St. John's College, Battersea, S.W.; and
Charles Nielsen, 15 Cliff Terrace, Hartlepool,

were balloted for and duly elected Fellows of the Society.

The following candidate was proposed for election as a Fellow of the Society, the name of the proposer from personal knowledge being appended :—

William John Greenstreet, M.A. (Cantab.), Head Master, Marling School, Stroud, Gloucestershire (proposed by Charles T. Whitmell).

Seventy-one presents were announced as having been received since the last meeting, including, amongst others :—

Rev. S. B. Burnaby, Elements of the Jewish and Muhammadan Calendars, presented by the author; W. de Sitter, Discussion of heliometer observations of *Jupiter's* satellites made by Sir D. Gill and W. H. Finlay, presented by the author; Taylor's General Catalogue of Stars for 1835·0, revised and edited by A. M. W. Downing, presented by the editor; *Astronomischer Jahresbericht*, Band 2, 1900, herausgegeben von

W. F. Wislicenus, presented by the editor; *Monthly Notices of the Royal Astronomical Society*, vol. 27, presented by W. C. Johnson; Photographs of the great Comet, 1901, presented by Sir David Gill.

The Great Comet of 1901, *as observed at the Royal Observatory, Cape of Good Hope.* By Sir David Gill, K.C.B., F.R.S., His Majesty's Astronomer at the Cape.

On April 24, at $2^h\ 54^m$ P.M., a telegram was received as follows:—

From Arthur Hill, Queenstown.

Royal Observatory, Cape Town:

"Saw a Comet this morning at 5 o'clock due East."

The following morning (April 24, astronomical time), the comet was seen by Mr. Innes, Mr. Lunt, and myself. Its position was first observed by Mr. Innes with the 10-inch guiding telescope of the astrographic equatorial, and afterwards by Messrs. Lunt and Innes with the 18-inch refractor of the McClean telescope. The nucleus was visible for some time after sunrise, but could not be followed as far as meridian passage. The observed places on this date depend on readings of the R.A. and Decl. circles, of which the index-errors were found by observations of the planet *Mercury*. The places given are corrected for refraction. On April 25 (astronomical date) there was dense fog on the eastern horizon, and the comet could not be seen. On April 26 similar circle observations to those of April 24 were secured with great difficulty by Messrs. Innes and Lunt on account of the strong light of the background of the sky.

On April 27 Mr. Lunt pointed the 6-inch equatorial to the ridge of the distant Hottentot Hollands Mountains at the expected setting in declination, and so saw the comet enter the field over the mountains, and thus obtained some readings of the circles, but the results are of doubtful value.

Cloudy weather intervened from April 27 till May 3, when the first accurate series of observations was secured by Mr. Innes, and no subsequent opportunity was lost by him. The preliminary results of reduction of all his observations are attached, together with an approximate orbit derived by him from his observations of May 3, 5 and 7.

Mr. Innes's drawing represents the comet as seen with the naked eye on April 24, the formation of the head and of the portions of the tail near the head being drawn with the assistance of the telescope.

1901 May 4

McCLEAN TELESCOPE

Exposure 15 minutes

1901 May 5

13 IN. ASTROGRAPHIC EQUATORIAL

Exposure 13 minutes

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1901 May 6
PORTRAIT LENS
Exposure 25 minutes

1901 May 7
PORTRAIT LENS
Exposure 25 minutes

Mr. Lunt's drawing of the comet on May 12 gives a very exact representation of the naked-eye view of this remarkable object.

It is desirable to defer a more detailed account of the physical appearance of the comet until our photographs, &c., have been properly reproduced and discussed.

Meanwhile the accompanying photographs and the notes of Mr. Lunt and Mr. Innes will be of interest to the Society.

Lantern Slides prepared from Negatives taken with the McClean Telescope.

*1. Taken 1901 May 4. Exposure 15^m.

2. „ „ 6. „ 10^m.

Lantern Slides prepared from Negatives taken with a Portrait Lens.

*3. Taken 1901 May 6. Exposure 25^m.

*4. „ „ 7. „ 25^m.

5. Copy of Mr. Lunt's drawing of comet on May 12.

Contact Prints from Original Negatives taken with the 13-inch Astrographic Equatorial.

No. 6300*a*.	May 4.	Exposure, 10^s at	$9^h\ 14^m$	sid. time.
6300*b*.	„ 4.	„ 20^s	$9^h\ 19^m$	„
*6302.	„ 5.	„ $13^m\ 44^s$	$9^h\ 15^m$	„
6305.	„ 6.	„ 15^m	$9^h\ 28^m$	„

The spectrum of the comet appears to be continuous; at least, with the means at our disposal, we have been unable to detect any bright lines. It unfortunately happened that, only a few days before the comet appeared, the large McClean spectroscope was sent off to England in order to have a new prism-box fitted.

Circle Readings.†

1901.	G.M.T. h m	R.A. h m s	Dec. ° ′	
April 24	16 37·2	1 29 55	+3 27·8	Innes, 10-inch telescope
	17 5·0	1 29 56·1	3 26 9	Lunt, McClean „
	17 31·4	1 30 14·5	...	„ „ „
	17 34·4	...	3 25·4	„ „ „
26	16 54·0	1 58 8·4	1 17·9	Innes, 10-inch telescope
	17 11·6	1 58 8·6	1 19·0	Lunt, McClean telescope
27	17 10·8	2 14 6·2	+0 24·8	Lunt, 6-inch telescope

* Reproduced. Plates 14 and 15.

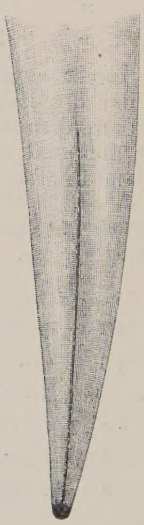
† All corrected for refraction.

*Equatorial Comparisons.**

1901.		G.M.T. h m s	R.A. h m s	Dec. ° ′ ″	
May	3	5 4 43	3 40 32·39	−0 32 18·6	Innes, McClean telescope
	4	5 15 0	3 54 29·23	−0 18 27·9	Innes, 7-inch telescope
	5	4 57 46	4 7 9·97	−0 1 32·4	,, ,, ,,
		5 29 24	4 7 25·23	−0 1 7·7	,, ,, ,,
	6	4 49 45	4 19 7·04	+0 18 23·3	,, ,, ,,
		5 5 48	4 19 14·91	+0 18 34·8	,, ,, ,,
		5 15 18	4 19 19·21	+0 18 37·1	,, ,, ,,
	7	5 1 15	4 30 24·20	+0 40 15·0	,, ,, ,,
	11	4 50 17	5 7 38·74	+2 13 45·1	,, ,, ,,
	12	4 49 56	5 15 22·65	+2 36 50·6	,, ,, ,,
		5 37 58	5 15 38·08	+2 37 39·3	,, ,, ,,
	13	4 59 28	5 22 38·71	+2 59 39·5	,, ,, ,,

Orbit.

T 1901 April 24·244
ω 202° 58′
Ω 110° 10′
ι 130° 44′
q 0·24251
Observations used, May 3–5–7.



Drawing by R. T. A. Innes. 1901 April 24.

* All corrected for refraction.

Observations by Mr. R. T. A. Innes.

1901 April 24. On account of conflicting telegrams, I had kept watch from about 15^h. About 17^h 30^m, when day was breaking, I had begun to despair of seeing any comet, but on giving a final look round in very bright twilight I saw two sheafs of light rising above the mountains in the east. A few minutes later the comet had entirely risen. It was a brilliant object with a bright nucleus and a tail about 10° in length, curved on the southern side. The colour of all was a very deep yellow, but the comet was very near the horizon. Through the 10-inch guiding telescope (now in broad daylight) the yellow tint of the nucleus was very marked. There was no coma visible, the tails (see drawing) springing directly from the nucleus. By comparison with *Mercury*, the nucleus was estimated to be two-thirds of *Mercury's* diameter, which makes it about 4″ ; its brightness was about equal to *Mercury's*.

When next seen with the 10-inch on April 26 the comet was very faint, but the nucleus did not seem smaller. On April 27 I could not find the comet, nor did I see it again until the evening of May 3, when the tail was quite altered. It now consisted of two nearly equal portions streaming from each side of the nucleus, not very unlike De La Rue's drawings of the comet of 1861, but the nucleus was round.

Evening Observations of Comet, 1901 *May* 3-*May* 12. *Mr. Lunt's description.*

The most remarkable feature of the comet, viz. the long faint preceding tail, did not become visible until the comet had emerged from the strong twilight. It was first seen on the evening of Friday, May 3, as a faint ray, scarcely distinguishable, springing from the head at an angle of about 40° to the main tail. This faint tail appeared on two photographs taken with a portrait lens the same evening. On the two following nights, however, as the comet receded further from the Sun and became visible against a darker sky, it was a most conspicuous feature. On the evening of Monday, May 6, the faint tail was seen to be quite four times as long as the main tail and fully 30° in length, but fading away so gradually that it was difficult to place any exact limit to it. At this time the comet attained its maximum splendour as a naked-eye object. With an exposure of 25 minutes a portrait lens showed not only the main faint tail, but two still fainter rays between it and the bright tail, clearly discernible in the lantern slide sent herewith.

The space on each side of the faint rays was filled with faint light, and the darker space between them showed clearly by contrast, although the two faint rays themselves were not so well marked to the eye as they appear in the photograph.

In the accompanying drawing I have endeavoured to represent the dimensions and most striking features of the comet as revealed both by eye observations and photographs. The position is that of the evening of Sunday, May 12, by which time the comet had become intrinsically much fainter, although as seen in a still darker sky it was yet a magnificent object.

The preceding side of the main tail was not then so markedly stronger than the following side as previously, but the tail still streamed off from each side of the nucleus in rays brighter than the space between them, which was filled with fainter light. The faint preceding tail was still fully 25° long, and reached, as shown in the drawing, as far as δ *Leporis*. The bright tail was about 7° long, and could be traced beyond ζ *Orionis* ; its fading beyond this point was much sharper than in the case of the faint tail.

Drawing by J. Lunt. 1901 May 12, 7.15 P.M. Cape Time.

The drawing shows the two short faint rays between the two main tails as they were photographed on May 6, but for clearness somewhat exaggerated in intensity.

The Oxford Photographic Determinations of Stellar Parallax. Reply to Professor Turner. By Sir David Gill, K.C.B., F.R.S., His Majesty's Astronomer at the Cape of Good Hope.

Professor Turner, in meeting my criticisms of the Oxford parallax observations, unfortunately does not touch the principal grounds for doubting the reliability of the Oxford results.

First of all Professor Turner does not refer to Pritchard's fundamentally unsound assumption that, by the methods which Pritchard employed, it is admissible to give independent results for the parallax of the principal star relative to each of two opposite comparison stars. It is obvious that as the scale-value is derived from the distance between the comparison stars a and b, it must be assumed either that their distance $a\,b$ is constant, or that it varies proportionally to the time. But, if the parallaxes of the comparison stars are not identical, the distance between the stars will vary with the season of the year, and consequently the correction for scale-value (*i.e.* the corrected observed distances) will vary proportionally to this change. In other words, if the comparison stars are situated systematically with respect to the principal star, we should, by Pritchard's methods, obtain the parallax of the principal star relative to the mean of the parallaxes of the two comparison stars, but we have no means of distinguishing, as Pritchard attempted to do, between the parallax of the principal star as derived by measures from star a and those from star b.

The obvious answer to such criticism is—then, why not take the means of Pritchard's parallaxes from the stars a and b, and accept the result as the true parallax of the principal star relative to the mean parallaxes of the stars a and b?

The answer to this question is given in a part of my criticism which Professor Turner omits to quote, and it runs as follows :—

"Although Pritchard frequently vaunts the novelty of his method, he takes no precaution to test its systematic accuracy. By simply taking photographs in the same season of the year at widely different hour-angles he might readily have ascertained whether the *apparent* parallaxes which he derived from observations six months apart, were really, in whole or in part, a function of the hour-angle at which the observations were made. It is obvious, for example, that any displacement of the apparent centre of the star's disc by chromatic dispersion of the atmosphere will, especially in the case of a bright star, be recorded on the photographic plate, and will not be eliminated, as in the heliometer observations, by the observer's superposing the

similarly coloured parts of the spectrum under observation. But amongst all the numerous plates taken and measured by Pritchard's assistants, one can find no published account of the application of any such simple and direct test."

It is the more extraordinary that Professor Turner should have neglected to reply to this criticism, because he seems alive to such sources of error when he says :—

"There may be very good reasons for using different scale-values in different directions quite independent of any hypothesis about distortion of the film. I mention one or two, quite on my own responsibility, not arising out of anything I have seen in Pritchard's work. 1. If the mirror or lens with which the photographs are taken is not an accurate surface of revolution (either essentially or because of flexure), its curvature in different directions, and therefore its focal length, will vary. There are obvious reasons why mirrors should err in this way more than lenses ; and it is a curious fact that plates taken with mirrors *do* show some variation in scale in different directions—*e.g.* the photographs of *Eros* taken with the 30-inch mirror at Greenwich (*Monthly Notices*, vol. lix. p. 13). If this is a *vera causa* Pritchard's rule would be essentially correct."

Now, in Pritchard's parallax work, practically all the observations in which the parallax factor has the + sign are made at eastern hour-angles, and all in which that factor has the — sign (from the same star of comparison) in western hour-angles ; or *vice versa*. If then, in Professor Turner's opinion (as also in my own), the flexure of the mirror may play an important part (especially where an angle of a few hundredths of a second of arc embraces the whole quantity to be determined), surely it is not too much to suspect that the effect of such flexure of the mirror may be to displace star-images by quantities which are not strictly symmetrical with the optical axis or with the image of the central star. Thus, as the direction of gravity with respect to the supports of the mirror is very different at eastern and western hour-angles, it was surely necessary to use the obvious precaution of taking photographs at the same season of the year both east and west of the meridian, in order to ascertain whether the apparent displacement of the principal star was due in whole or in part to the effects of flexure in the mirror or to such other causes as I have already indicated.

But Professor Turner's paper reveals other sources of possible error of which no mention is made by Pritchard, and which no one could have foreseen without access to the details of the original observations. For example, he states "that, for the period June 23 to July 1, *when a different mirror was in use*," &c. (the italics are mine). It would be interesting to know if different mirrors were used on other occasions, and what pre-

cautions, if any, were taken to replace the original mirror precisely in the same position relatively to its supports ; these are points on which the published results are entirely silent, although they are of crucial importance in estimating the reliability of the results.

As to the figures given by Professor Turner, they apparently present, in the form in which he arranges them, a better agreement than my original inspection of Pritchard's tables led me to think. Unfortunately for the moment our copies of the Oxford observations are in the hands of the binder in England, and I am unable to examine them. But whatever the result of such a re-examination might be, it cannot alter the conclusion that no reliance can be placed on the results, because they afford no data which can serve to distinguish between the displacements caused by instrumental or refraction effects and those which may be due to parallax.

Postscript.—Since the above was written my books have been returned from England, so that I am in a position to complete a reply to Professor Turner's note.

Professor Turner, having rearranged Pritchard's Table XIII., not in order of date but in order of the mean value of the two corrections, appeals to an agreement of the majority of the signs as an answer to my general criticism, viz. "that throughout the series there is little evidence of systematic agreement even in the signs of the scale-value corrections, and their average discordance is very much greater than the systematic error of observation assigned by Professor Pritchard."

The table in question gives, *not* the actual discordances between the measures, but the effect of a difference between the mean scale-value and the instantaneous scale-value as independently determined from measures of the diagonals $a\ b$ and $c\ d$ (for a distance=1000″).

Where the instantaneous scale value is grossly different from the mean scale-value there is a perfect agreement of sign, for it is obvious that by focussing badly enough or in any way adopting a sufficiently erroneous mean scale-value, one could always secure a similarity of sign in the instantaneous scale-value corrections from the two diagonals. Even as matters stand we find that out of 87 pairs of scale-value corrections, the components of which should have the same sign, no less than 24 have opposite signs. To examine the matter more closely, let us take simply the differences between the instantaneous scale-values derived from the stars $a\ b$ and $c\ d$. These are given in the following table :

Difference of Scale-value (on a distance of 1000″) derived from measures of the diagonals a b and c d.

Values of ($a\,b - c\,d$).

1886.							
May 30	+0″·306	Sept. 15	−0″·111	Dec. 2	−0″·050	Mar. 16	−″·013
June 1	+ ·098	16	+ ·166	4	− ·233	23	−0·216
4	− ·306	17	− ·041	7	− ·048	27	+ ·029
8	+ ·183	18	+ ·090	9	− ·147	Apr. 2	+ ·035
15	− ·334	20	− ·056	14	− ·078	16	+ ·402
16	+ ·213	22	+ ·170	16	− ·372	19	− ·290
23	+ ·592	27	+ ·098	24	− ·372	20	− ·226
24	+ ·269	29	+ ·079	1887.		25	− ·097
28	− ·060	30	+ ·278	Jan. 5	− ·090	26	− ·083
30	+ ·591	Oct. 2	− ·073	8	− ·112	29	− ·072
July 1	− ·171	6	− ·080	10	+ ·009	30	− ·007
Aug. 20	− ·034	13	− ·004	12	− ·138	May 5	+ ·005
24	+ ·049	21	− ·050	20	− ·044	7	+ ·047
26	− ·124	22	− ·036	25	− ·022	9	+ ·002
28	+ ·274	Nov. 3	− ·034	31	− ·094	10	+ ·071
29	+ ·040	5	+ ·087	Feb. 5	− ·100	13	− ·065
30	+ ·079	16	− ·173	8	+ ·071	14	− ·176
31	+ ·014	17	+ ·066	17	− ·067	16	− ·329
Sept. 7	− ·283	18	+ ·028	25	+ ·054	18	− ·019
10	+ ·158	23	− ·200	26	− ·060	20	− ·277
11	− ·074	29	+ ·018	27	+ ·133	26	− ·098
13	− ·233	Dec. 1	− ·004	Mar. 12	− ·097	31	− ·048

If the observations were perfect all the quantities of the above table would be zero ; let us examine and see how far the errors of the table correspond with Pritchard's estimate of the accidental errors of his observations, or how far they contain other sources of error.

Pritchard's derived values of the probable accidental error of observation are the following :—

For Star	a	to	61_1 Cygni	±	0″·091 (p. 17)
,,	a	,,	61_2 ,,	±	·100 (p. 24)
,,	b	,,	61_1 ,,	±	·115 (p. 31)
,,	b	,,	61_2 ,,	±	·100 (p. 36)
,,	c	,,	61_1 ,,	±	·102 (p. 47)
,,	c	,,	61_2 ,,	±	·088 (p. 52)
,,	d	,,	61_1 ,,	±	·089 (p. 58)
,,	d	,,	61_2 ,,	±	·104 (p. 64)
			Mean	±	0″·100

Now the sum of the squares of our table of 87 differences is

$$2{\cdot}8082.$$

The *mean error* of a single difference between the scale-values derived from $a\ b$ and $c\ d$ is therefore

$$\sqrt{\frac{2{\cdot}8082}{87}} = \pm\ 0''{\cdot}180$$

or its probable error $\pm\ 0''{\cdot}120$.
But the distance $a\ b$ is $= 2382''$
$c\ d$ $= 2066''$

hence the probable error of the discordance of the distances $a\ b$ and $c\ d$, if reduced by an instantaneous scale-value derived from observations of both pairs, would be

$$\pm\ 0''{\cdot}120 \times 2{\cdot}224 = \pm\ 0''{\cdot}267$$

or the probable error of the measure of the single distance $a\ b$ or $c\ d$ would be

$$\frac{0''{\cdot}267}{\sqrt{2}} = \pm\ 0''{\cdot}190.$$

Now why should this extraordinary difference exist between the probable error of a measure when, on the one hand, the scale-value is determined along a line nearly coincident with the direction of measurement, and when, on the other hand, the scale-value is determined by the mean of two standard distances at right angles to each other?

This fact is the more remarkable because the smaller probable error, $\pm\ 0''{\cdot}10$, is derived from measures between two images, one of which is that of the large and less sharply defined disc of the principal star, whilst the larger probable error, $\pm 0''{\cdot}190$, is derived from measures between the smaller and sharper discs of the comparison stars.

There is only one possible explanation, viz. that some additional fault or cause of error, other than that of accidental error of pointing, is introduced, whose probable effect is

$$\sqrt{0{\cdot}19^2 - 0{\cdot}10^2} = \pm\ 0''{\cdot}162.$$

And yet Professor Turner writes as if this error were non-existent, for he remarks "that Sir David Gill sets up this imaginary fault and proceeds to explain it by an equally imaginary cause"!

It is hardly desirable to treat as "imaginary" sources of error by which the amount of purely unavoidable accidental error of pointing is nearly doubled. It seems preferable to endeavour to trace their origin to some cause.

It is all very well arbitrarily to dismiss distortion of the film in the way which Professor Turner says that Pritchard did, viz.

"because he did not believe in this distortion;" that is a very simple way of glossing over a difficulty, but it is not a good plan for getting at the truth. It is also very easy for Professor Turner to deny that distortion of the film can exist; he gives no proof to that effect, but, strangely enough, he next proceeds to give an explanation of the fault which he had just before dismissed as "imaginary" as follows:—

1. "If the mirror or lens with which the photographs are taken is not an accurate surface of revolution (either essentially or because of flexure) its curvature in different directions, and therefore its focal length, will vary. There are obvious reasons why mirrors should err in this way more than lenses; and it is a curious fact that plates taken with mirrors *do* show some variation in scale in different directions—*e.g.* the photographs of *Eros* taken with the 30-inch mirror at Greenwich (*Monthly Notices*, lix. p. 13). If this is a *vera causa* Pritchard's rule would be essentially correct."

2. "If the plate is not strictly normal to the axis of the telescope, there would be variation in scale in different directions. But in this case Pritchard's rule would not be quite correct. If, for instance, the twist was about the line joining *a b*, then the scale-value would remain unaltered for the direction *a b*, and would be altered in the direction *c d*; but the distance from *c* to the star might be increased, while that from the star to *d* was diminished."

3. "If the distances on the plate in the direction *a b* were *measured on a different day* from those in the direction *c d*, then Pritchard's rule would be essentially correct. The small variations would then depend partly on different expansions of the plate with temperature."

With regard to No. 1, Professor Turner apparently thinks that if, from any cause, the focal length of the mirror were different in two directions, the scale-value of a plate, normal to the optical axis of the mirror, would be different in these two directions. This is a mistake, for if the star images are symmetrical the scale-value is defined by the distance of the sensitive surface of the plate from the mirror, and by that only, and the scale-value is quite independent of the focal length. The effect of flexure or dissymmetry of the mirror would only be to produce ill-defined and probably unsymmetrical images in both directions, in pointing on which, errors would be produced in the measures of the standard distances, but such errors would not be linearly proportional to the distance, and consequently Pritchard's rule in that case is incorrect.

With regard to No. 2, Professor Turner's conclusions here are perfectly correct, but unfortunately Pritchard has given no information as to the mode in which the plate-holder is supported or the process by which the plate was adjusted normal to the axis of the mirror. If the mounting of the plate-holder was

reasonably rigid it is improbable that this adjustment would change sensibly between one night and the next for plates taken nearly at the same hour-angle, and yet large differences (far exceeding the probable errors of pointing) do occur in the scale-values derived from the two diagonals under these circumstances. Professor Turner's hypothesis fails to explain this. On the other hand, when the instrument is reversed (as it was at opposite parallactic epochs), it is not impossible that the adjustment of the plane of the plate with respect to the axis may have been changed to a small extent by flexure of the support of the plate-holder.

There are long periods—*e.g.* December 1 to February 5—in which the difference of scale-values $a\,b - c\,d$ (with one small exception) have the same sign, and this may possibly be due to a temporary maladjustment of the plate to the normal plane during that period. Unfortunately Pritchard's published results afford no mention of the dates when such adjustments (if any) were made, and no definite conclusions can be drawn. It is clear, however, that if the plate was liable to such errors the resulting parallaxes would be quite unreliable.

With regard to No. 3, Professor Turner says that if the two diagonals $a\,b$ and $c\,d$ were measured on different days the variations between the scale-values derived from $a\,b$ and $c\,d$ might depend on the different expansions of the plate by temperature. That may be true if we could admit the possibility of sufficient change of temperature, but even then it is only partly true, because Professor Turner omits to take into account the expansion of the steel screw of the De la Rue Macro-Micrometer in terms of which the plates were measured. Changes of temperature would therefore produce changes in measurement depending on the difference between the coefficients of expansion of steel and plate-glass. The difference of these coefficients is

0·000003 per degree Centigrade,

corresponding, on a distance of 2000″ to 0″·006 per degree C. Thus, to produce a difference equal to the probable value of the error which we seek to explain, viz. $\pm$ 0″·19, it would be necessary that the two diagonals should be measured at temperatures differing by 32° Centigrade = 58° Fahrenheit. Is Professor Turner accustomed to submit his assistants to changes of temperature of this character in the measuring room? Pritchard apparently was not, for he says (*Mem. R.A.S.* xlvii. p. 4), "It was also found that, within the range of the small variations of temperature under which the instrument is used, no correction is required within the limits of the error of observation." On the same page Pritchard also states that "the temperature at the time of observation was noted," so that, although no records of these temperatures are published, Professor Turner can satisfy himself by inspection of the original records whether what Pritchard considers "a small variation of temperature" amounts to 58° Fahrenheit. But even if such uncomfortable treatment was

meted out to Pritchard's assistants, it must be remembered that each result is the mean of the measurement of four plates, and therefore Professor Turner's hypothesis would require the still more improbable assumption that in the mean all the $a\ b$ measures were made when the assistants were shivering at 32° Fahrenheit, and all the $c\ d$ measures when they were perspiring at 90° on the same scale, or *vice versa*.

The inadequacy of all Professor Turner's explanations is thus sufficiently proved. I therefore still venture to think that my hypothesis of unequal distortion of the film in different directions is, to say the least of it, a more probable cause of the errors in question.

But, after all, such distortion of the film may, by an easy-minded astronomer, be treated as an accidental source of error in the mean of many plates, although such an assumption does not touch the main grounds of my distrust of the Oxford parallaxes, viz. that, for the reasons given in the first part of this paper, the published results afford no proof whatever that the apparent parallaxes determined by Pritchard are not, in part at least, a mere function of the hour-angle at which the observations were made.

In conclusion, I take advantage of this opportunity to correct a mistake which I have made in the Introduction to vol. viii. part II. of the *Annals of the Cape Observatory*. I have there stated (p. xv) :—

"A precision, at least equal to that of Heliometer observations, has been claimed for the photographic method of determining Stellar Parallaxes. But, apart from their systematic errors, this is very far from being the case in the Oxford measures, even if we accept Pritchard's own result for their probable error, viz. $\pm$ 0″·10 for the single observation.

"The fact is apparently overlooked, that, for the single distances of which the residuals are discussed at Oxford, the maximum parallax factor is 1, whereas in the difference of two opposite distances, as discussed in the modern Heliometer method, that factor is 2. In other words, if the mean of the squares of the Heliometer residuals was the same as that of the Oxford residuals, one Heliometer observation would have four times the weight of one Oxford observation. But we have found (page 144 B) that the probable error of one observation derived from Gill's residuals is $\pm$ 0″·071 as compared with $\pm$ 0″·100 for Oxford ; their corresponding weights with equal parallax factors would therefore be as 2 : 1. Thus, having regard to the weight of the corresponding parallax factors, it would require measures of *eight* Oxford plates – even supposing them free from systematic error—to have the same weight in the determination of parallax as one of Gill's Heliometer measures."

My mistake in this criticism is in having overlooked the fact

that each of Pritchard's results depends on four plates, and not on a single plate as is there assumed.

The amended conclusion of my criticism should therefore be that it requires measures of thirty-two Oxford plates—even if they were free from systematic error—to have the same weight in the determination of parallax as one Heliometer observation.

The Oxford Photographic Determinations of Stellar Parallax. Further Reply to Sir David Gill. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

Sir David Gill kindly sent me a copy of the above reply, and it may be convenient to have my rejoinder read to the Society at the same time as his paper.

In his volume on stellar parallax Sir David Gill gave three reasons for regarding the Oxford determinations as not "of proved value," which may be briefly stated as follows :

(1) That Pritchard gave separate results for parallax from two comparison stars *a* and *b*, although the distance *a b* had been used to correct each measure for scale-value.

(2) The "chromatic dispersion" objection.

(3) That the figures published by Pritchard gave evidence of distortion of the film, sufficient to render the observations of small value.

In a former paper (*Monthly Notices*, lxi. 5) I considered the third objection only, for as it deals with an actual matter of fact it seemed to me by far the most important. Sir David Gill's reply is chiefly in his postscript, which I will consider presently.

As regards the first and second points which Sir David now pronounces the more important, I am again at variance with him. To the first he has himself supplied "the obvious answer." Surely there is no harm done in giving the results separately? If they are not independent the mean can be taken as Sir David Gill says ; and to exhibit them separately has the advantage of showing that there is no numerical mistake, if nothing more.

As regards the second point (between which and the first Sir David Gill traces some connection which I fail to see), it is quite true that Pritchard made no experiments on the effect of chromatic dispersion. It is equally true that Sir David Gill had made none himself at that time, though he had also published parallax work.

The discussion about chromatic dispersion of the atmosphere has come up since then. Indeed, it was not until 1898, when Pritchard had been dead five years, that Sir David Gill initiated

observations to test its reality as a disturbing cause on heliometer observations; and when these were made the whole thing was found to be something very like a mare's nest. Sir David Gill says in his Introduction (*Cape Annals*, vol. viii. pt. II., p. viii.): "I may state at once that the results of these observations go to show that the observer in the process of measurement, unconsciously superposes, *not* the most brilliant points of the two short stellar spectra formed by the chromatic dispersion of the atmosphere, but the two similarly coloured parts of these spectra, and thus the effects of chromatic dispersion are entirely eliminated."

Now here we have in the same breath two distinct things—an observed fact and Sir David Gill's inference from it. The *observed fact* is *merely* that the observations are not affected. The inference as to *why* this happens is not absolutely proved. Atmospheric chromatic dispersion is represented by a quantity $\Delta\beta$, the value of which is as yet undetermined. Sir David Gill has "suggested a method of determining the degree of redness of a star" (he gives, however, the wrong reference, and I have not been able certainly to identify the right reference; in *Cape Annals*, vol. viii. pt. II., p. 125B the reference is given as *Monthly Notices*, 1898 December, p. 98. It is, perhaps, 1897 December, p. 68 ?), but he has apparently got no further than suggesting a *method for determining* $\Delta\beta$. Now I submit that until we know more about the value of $\Delta\beta$ there are two alternative inferences (at least) from the fact that the observations made on δ *Sagittarii* with a heliometer show no effect. One possibility is that $\Delta\beta=0$ to the order required—that chromatic dispersion does not sensibly affect such observations either heliometric or photographic. Sir David Gill prefers to say: We know that $\Delta\beta$ must be sensible, and as it does not apparently affect heliometer observations there must be something in the method of observation which eliminates it. His experience with the heliometer entitles this opinion to the greatest respect, but we must be careful to remember that it is as yet *only* an opinion, and that there are other ways of explaining the observed fact. Therefore it is not quite "obvious" * that photographic observations will be affected just as surely as heliometer observations are not. The experiment has still to be tried, and it is, to say the least, quite possible that atmospheric chromatic dispersion affects photographic parallaxes as little as heliometer parallaxes. Theorising beforehand has been proved misleading in the latter case (although not until a year or two ago) and may be just as misleading in the former.

* Sir David Gill writes: "It is obvious, for example, that any displacement of the apparent centre of the star's disc by chromatic dispersion of the atmosphere will, especially in the case of a bright star, be recorded on the photographic plate, and will not be eliminated as in the heliometer observations, by the observer superposing the similarly coloured parts of the spectra under observation." We have here several assumptions which are by no means obvious.

With regard to the postscript in which Sir David Gill deals with the third point, it is almost sufficient for my purpose to compare his original statement with that which he now substitutes. The original statement was: "Throughout the series there is little evidence of systematic agreement even in the signs of the scale-value corrections." That of his postscript is now: "Even as matters stand we find that out of 87 pairs of scale-value corrections, the components of which should have the same sign, no less than 24 have opposite signs." (The 24 should be 20: I have counted very carefully both in the original table and in my rearrangement of it, *Monthly Notices*, lxi. p. 310.)

Now if Sir David Gill regards these statements as equivalent we have reached the point at which this discussion may stop—where we must "agree to differ." To me there is such an essential distinction between them that I should have expected from him something like a withdrawal of the original statement. With a series of small quantities all near zero a certain number of accidental differences of sign is inevitable; and of the 20 pairs the following 8 for example may be regarded as sensibly zero in both members:—

Aug. 30	+0".041	−0".038
Oct. 2	−0".003	+0".070
Oct. 21	−0".046	+0".004
Nov. 17	+0".031	−0".035
Nov. 29	+0".016	−0".002
Dec. 2	−0".047	+0".003
Apr. 26	−0".048	+0".035
Apr. 29	−0".011	+0".061

Of the remaining 12 five belong to the 11 observations at the beginning of the series where I have already explained that there is an error [I may, perhaps, reassure Sir David Gill as to the recurrence of such errors: from August 20 the mirror was not changed], which leaves only seven sensible differences of sign in the 76 observations after the work was satisfactorily begun.

A word as to the phrases "when the instantaneous scale value is *grossly* different from the mean;" "by focussing *badly* enough;" "a sufficiently *erroneous* mean scale-value." Sir David Gill uses these phrases to characterise differences of scale-value amounting in extreme cases during a year to 0".6 in 1000", or ·0006 of the unit. May I ask him to examine the scale-values of the Astrographic Chart plates which he is now taking and measuring at the Cape? Or will he perhaps kindly look at figures already published, say those in *Monthly Notices*, lv. pp. 62 and 63? Here we see compared the scale-values on four plates taken at Greenwich on the *same* night—not of the same region, but of regions overlapping sufficiently well to give a good

comparison—and the scale-values, indicated by the constants a and e, differ to fully half the extent shown in Pritchard's results. It may be remarked that the mere *change of measurer, keeping the plates the same*, can give an apparent change of scale-value from +·00022 to −·00017 (see the values of e for plates 2059 and 2057 *loc. cit.*). In Pritchard's Table XIII. these quantities would appear as +0″·22 and −0″·17. If, then, we get such accidental differences for plates taken within a couple of hours on the same night with a refractor (which is known to be more manageable than a reflector), after all the precautions of agate-stops &c. which were devised by the united wisdom of an international conference, and *when* 30 *or* 40 *stars are used* ; it seems to me that Pritchard's range of differences for plates taken all the year round with a reflector, as pioneer work, *and with only two stars measured*, is not excessive ; and that Sir David Gill's epithets are quite out of place.

I pass to the new examination of Table XIII. undertaken by Sir David Gill in his postscript. He finds the probable error of a measure of distance from Table XIII. and compares it with that found elsewhere by Pritchard (p. 65 of Pritchard's *Memoirs* is better than the scattered references quoted by Sir D. Gill). I remark in reply :—

(1) If the eleven observations May 30 to July 1, in which there is an error (as stated in my former paper) be omitted, the sum of the squares is reduced from 2″·81 to 1″·62 ; and (following Sir David Gill's further work) the probable error of a single distance ab or cd would be ± 0″·147 instead of ± 0″·190 If we are trying merely to make out a strong case against Pritchard we might of course insist on retaining all the printed figures ; but if we are really trying to find out the truth of the matter, it is better to exclude figures known to be erroneous.

(2) Sir David Gill assumes apparently that the probable error of a distance is independent of the magnitude of the distance measured. It seems more likely that it is greater for large distances than for small ; and this view is confirmed on reference to the figures. Pritchard gives in various tables the "average deviation" of the measured distances ; and, on adding up corresponding tables, we find that the average deviation for the half-distances from the central star to the four stars a, b, c, d is about 0·83 times * that for the whole distances ab and cd ; and this in spite of the fact that "the smaller probable error" (to quote Sir David Gill's words) "is derived from measures between two images, one of which is that of the large and less

* In Tables XX., XXII., VIII., X., XVII., XV., V., II., XIV., I., Pritchard gives comparable "average deviations" for distances of 955″, 963″, 1002″, 1022″, 1107″, 1115″, 1360″, 1380″, 2065″, and 2380″. The means for the first eight tables are much the same, viz. 0″·183, 0″·175, 0″·191, 0″·199, 0″·183, 0″·189, 0″·185, 0″·185, mean of all 0″·186. The means for the last two are 0″·219 and 0″·229, mean of both 0″·224. Ratio of 0″·186 to 0″·224 is 0·83.

sharply defined disc of the principal star, whilst the larger is derived from measures between the smaller and sharper discs of the comparison stars." Indeed, by using this factor 0·83 as it stands, we shall not only reduce the probable error for a distance of 2200″ so as to be properly comparable with one of half that distance, but we shall at the same time compensate directly the unknown disturbance of the results due to the large disc of the central star, which Sir David Gill leaves as an outstanding balance in his own favour. This factor 0·83 then reduces our probable error found from the comparison of *ab* and *cd* still further from 0″·147 to 0″·122, and the probable effect of the "additional fault or cause of error" is reduced from $\sqrt{(0''\cdot19)^2-(0''\cdot10)^2}=\pm0''\cdot16$ to $\sqrt{(0''\cdot122)^2-(0''\cdot100)^2}=\pm0''\cdot07$ which is a sort of quantity one might quite reasonably expect.

(3) Accepting this remainder as a real difference, I must still demur to Sir David Gill's statement that I have written as if this difference (he calls it an "error") were non-existent. On the other hand, I spent some paragraphs in considering possible causes for it, which he has quoted. When I spoke of an "imaginary fault" I was referring to quite another matter, viz. the fault originally assigned to the observations by Sir David Gill in saying that "there is little evidence of systematic agreement even in the signs." I have already pointed out that his revised statement ("we find that out of 87 pairs no less than 20 have opposite signs") is very different from the former; and there is the same wide difference between the "fault" I called imaginary and the discrepancy, now reduced to an amount not surprising, for which I have already assigned three other possible contributing causes at least. And, if Sir David Gill wishes, I will admit as a possible fourth the "distortion of the film." I have called it an "imaginary cause," and will still do so; for its sensible existence has still to be proved. That it cannot be the sole or even the chief cause of the discrepancy under consideration follows from the fact to which Sir David Gill himself draws attention, viz. that "there are long periods in which the differences of scale value have the same sign." It will require some ingenuity to explain how distortion of the film can act in this way.

Of the other three causes suggested by me let me take first that which enables Sir David Gill to contribute to the gaiety of nations—I allude to the "freezing and perspiring" paragraphs. It seems sad to have to knock the bottom out of a joke; for we get so few in astronomy, especially in such serious work as parallax work. But the whole point of this joke lies in its coefficient, and I cannot accept the coefficient. First of all, where does Sir David Gill's 0″·19 come from? Surely he means $\sqrt{(0''\cdot19)^2-(0''\cdot10)^2}$ or 0″·16 as above? He would probably have corrected this in proof (as also the 24 for 20 earlier mentioned. I should have gladly corrected these slips for the distant author, but he has rendered this course impracticable by

laying such stress on the figures), and then he would have contracted his range of 58° F. to 49° F. But a further contraction is necessitated by the little revision of figures I have sketched above—bringing the 49° down to 21°. I must finally submit that temperature is by no means called upon to explain the *whole* discrepancy; half or a third will be quite a respectable contribution, and so we get down to 10° or 7° perhaps, which is quite sober earnest.

Next as regards the tilt of the plate: "If the mounting of the plate-holder was reasonably rigid it is improbable that this adjustment would change sensibly between one night and the next for plates taken nearly at the same hour-angle." Sir David Gill is making no allowance for the variable curvature of the plate, which is an important factor in these matters. Once again I would refer him with confidence to his own results or to such published figures as have been already quoted; and I will ask him to remember that the mounting of a plate-holder in a reflector cannot be so rigid as in a refractor. Finally, my suggestion about the curvature of the mirror he calls a mistake. Possibly; but I am not convinced by what he says. The point is not an easy one, and actual experiment will be the best test; but I will venture to put down the way in which it appears to me. If scale value has any uniform meaning, we may suppose the mirror replaced by a pinhole, from which the rays diverge to the plate. The scale value will then depend simply on the distance of this pinhole from the plate. Now *where* is this pinhole? Is it absolutely *at* the surface of the mirror? It must be close to it, but I do not feel at all sure that it is not at a small distance from it. We can scarcely decide by simple theory; for the measurement of images and even the actual meaning of the word "focus" are matters of compromise, on lines only imperfectly worked out theoretically; and the position of this hypothetical pinhole depends on them. Hence, speaking tentatively, I would point out that if the place of this representative pinhole is not rigidly *at* the surface of the mirror, its distance from it must depend on the curvature (there is nothing else for it to depend on); consequently, if the curvature is different in different directions we shall have different representative pinholes for these directions, *i.e.* different scale values. Experiments would settle the point, but it is not easy to settle it theoretically. To prevent possible misunderstanding, I may say that I consider all the following points to be proper subjects for experiment:

(1) Photographic distortion of the film.
(2) Atmospheric chromatic dispersion.
(3) Effect of flexure of mirror on scale value.

Of none of these have I intentionally denied the *possible* existence; I will even affirm their existence. But to what extent? The whole point is, What is their magnitude?

The discussion has already reached a greater length than I anticipated; for, as I have already said, it is not my intention to undertake a systematic re-examination of Pritchard's work. Starting such work now one would do it differently perhaps; we have had ten years' experience in photographic measurement since Pritchard began. But that his work can be dismissed in the cursory manner of Sir David Gill's Introduction I do not believe. It is at least worthy of careful examination. For one thing it is (so far as I know) even yet the only considerable research on stellar parallax by photography actually carried out. Many peeple have talked of doing such work before and since, but as compared with talk, an actual piece of work done is entitled to a measure of respect.

The Normal Equations that arise in the usual Schemes of Observation for Division Errors and their Solutions. By P. H. Cowell, M.A.

Notation.

The circle of 360° is supposed divided into n arcs of $\frac{360°}{n}$ each; and the error of the division $r\,\frac{360°}{n}+\mathrm{A}°$ is denoted by x_r, where r takes all the values 1, 2 . . . n. At the first setting of the circle the $s+1$th division at the $\mathrm{A}+(s+1)\,\frac{360}{n}$th degree is supposed to be under the sth microscope. In some cases s will take all integral values from 1 to n, but in other cases s will only take some of these values. Between the settings the graduated circle is supposed to be turned in such a way that the $s+t$th division at the $\mathrm{A}+(s+t)\,\frac{360}{n}$th degree is under microscope s. The series of observations will be complete when there have been n settings of the circle, but in order to eliminate time changes it is usual to at once repeat the observations in the reverse order. Any uniform change is eliminated either by taking the mean of corresponding readings, that is to say, readings equidistant from the middle of the double series, or preferably by solving each single series separately, and taking the mean of the final results, whose discordance indicates the amplitude of the changes that may have occurred.

It is henceforth assumed that a single series of observations is being dealt with.

The division error x_r is defined as the amount by which the circle appears to read too large in consequence of division error. The sth microscope is supposed subject to an error of position

y_s, causing the reading under the microscope to be too large. At the tth setting the circle will be supposed to have been turned too far by a distance z_t, so that all the readings at the tth setting are on this account z_t larger than was intended.

The Equations.

If $c_{s\,.\,t}$ be the reading under the sth microscope at the tth setting, the typical equation is

$$x_r+y_s+z_t=c_{s\,t}$$

where

$$r=s+t$$

and r or t may have any integral value from 1 to n, and s may have certain selected values according to the number and positions of the microscopes. Let m be the number of microscopes; then we have mn equations of the above type to determine $2n+m$ quantities, of which two, say x_n and z_n, are arbitrary. If, therefore, mn is greater than $2n+m-2$, we have more equations than unknown quantities, and theoretically normal equations should be employed. Since, however, the normal solution is the best, that is to say, of maximum accuracy, any solution, where the fundamental equations are combined not very differently, will have an accuracy hardly appreciably less; there is, on the other hand, a risk that the fundamental equations, if combined arbitrarily, may be combined in a way that may give a very erroneous result. As an illustration, if $x=1$ and $x=3$ be two equations of equal weight, the normal solution gives $x=2$; but any other value for x may be obtained by unequal weighting. To return to the system of equations under discussion, if all the quantities except x_r be eliminated in an arbitrary fashion is there any guarantee that the process is not equivalent to assigning a negative weight to some of the equations?

The Normal Equations.

The normal equations formed in the usual manner are:

$$\begin{aligned} mx_r &+\Sigma y+\Sigma_s z_{r-s}=\Sigma_s c_{s\,.\,r-s}\\ \Sigma x &+ny_s+\Sigma z =\Sigma_t c_{s\,.\,t}\\ \Sigma_s x_{s+t}&+\Sigma y+mz_t =\Sigma_s c_{s\,.\,t}\end{aligned}$$

When the suffix of Σ denotes the symbol for all values of which the summation is to be taken, and when the suffix is omitted, the summation includes all quantities expressed by the same letter.

It is to be remarked that there are $2n+m$ equations written above to determine $2n+m$ quantities of which two are arbitrary. As, however, the sum of all the equations in a group is the same

for each of the three groups, there are only $2n+m-2$ independent equations.

As the y's do not occur in the first and third groups except in the single form Σy, it is clear that we may treat the m equations of the second group as determining the m quantities y; we may therefore suppress this group, and we are left with

$$\begin{aligned} mx_r \quad &+\Sigma y+\Sigma_s z_{r-s}=\Sigma_s c_{s\,.\,r-s} \\ \Sigma_s x_{s+t}+\Sigma y+mz_t \quad &=\Sigma_s c_{s\,.\,t} \end{aligned}$$

or $2n-1$ independent equations, to determine n quantities x, n quantities z, and one quantity Σy, or $2n+1$ quantities in all, of which two, x_n and z_n say, are arbitrary.

Particular Cases.

(i.) $n=6$; $m=6$.

The normal equations are

$$\begin{aligned} 6x_r&=\Sigma_s c_{s\,.\,r-s} \\ 6z_t&=\Sigma_s c_{s\,.\,t} \end{aligned}$$

where the arbitrary constants have been chosen so that

$$\Sigma x=\Sigma z=-\Sigma y.$$

The most convenient form for computation is to enter the 36 readings in a square of six by six, the readings in the same row corresponding to the same setting of the circle, and the readings in the same column to the same division of the circle, and the readings of the same microscope will fall on a line parallel to one of the diagonals.

Let S denote a sum taken vertically, and S′ a sum taken horizontally; then

$$6x_r=\mathrm{S}_r \qquad 6z_t=\mathrm{S}'_t$$

and as a numerical check S′S=SS′, or the sum taken horizontally of the sums S is equal to the sum taken vertically of the sums S′.

If the weight of a single reading be unity, the weight of a determination of division error is 6.

(ii.) $n=8$; $m=7$.

Let the missing microscope be the last one; the case supposed sometimes occurs when it is inconvenient or impossible to mount an eighth microscope.

The normal equations are

$$\begin{aligned} 7x_r-z_r \quad &=\Sigma_s c_{s\,.\,r-s} \\ -x_t+7z_t&=\Sigma_s c_{s\,.\,t} \end{aligned}$$

where the arbitrary constants are chosen so that

$$\Sigma x=\Sigma z=-\Sigma y.$$

Arrange the 56 readings in a square of eight by eight with a diagonal empty to correspond to the missing microscope. As before, readings on the same row correspond to the same setting ; readings in the same column to the same division. Let S denote the sum of the seven quantities in a column, S′ the sum of the seven quantities in a row ; then

$$7x_r - z_r = S_r$$
$$-x_r + 7z_r = S'_r ;$$

then eliminating z_r

$$48x_r = 7S_r + S'_r$$

To finish the computations, multiply the quantities S by 7 in a row beneath them, and add the quantities S′. As a check S′S=SS′ as before ; also $48\Sigma x = 8S'S$.

If the weight of a single reading be unity, the weight of a determination of division error is $\frac{1152}{175} = 6{\cdot}58$.

(iii.) $n=24$; $m=8$, s taking the values 1, 4, 8, 12, 13, 16, 20, 24. This is a usual way of obtaining the division errors for 15°. It is usually preceded by a determination of every 60°. In fact, by suppressing the readings of the microscopes $s=1$, $s=13$, we arrive at four repetitions of case (i.), so that the division errors of four sets of divisions, each set being a group of six 60° apart, may be determined as in case (i.) each to an arbitrary constant. It is the function of the two microscopes $s=1$, $s=13$ to connect the four arbitrary constants of the four groups.

The normal equations are—

$$8x_r + \Sigma y + \Sigma_s z_{r-s} = \Sigma_s c_{s\,.\,r-s}$$
$$\Sigma_s x_{s+t} + \Sigma y + 8z_t = \Sigma_s c_{s\,.\,t}$$

The eliminations, in order to obtain the values of the quantities X separately, are difficult to perform, and therefore, for the reasons given above, equations for four quantities X_p ($p=1, 2, 3, 4$)

where $$X_p = \Sigma_k x_{4k+p} \quad (k=1, 2, 3, 4, 5, 6)$$

will be formed. A similar meaning being attached to the symbols Z_p and $C_{s\,.\,p-s}$ and $C_{s\,.\,p}$, we have, by adding the normal equations in groups of six—

$$8X_p + 6\Sigma y + \Sigma_s Z_{p-s} = \Sigma_s C_{s\,.\,p-s}$$
$$\Sigma_s X_{s+p} + 6\Sigma y + 8Z_p = \Sigma_s C_{s\,.\,p}$$

Now
$$\Sigma_s Z_{p-s} = 6Z_p + 2Z_{p-1}$$
$$\Sigma_s X_{s+p} = 6X_p + 2X_{p+1}$$

Again, the $24 \times 8 = 192$ readings of the complete set may be grouped as follows : Four groups of 36 readings each, namely, the readings of the six symmetrical microscopes at the pth, $p+4$th, $p+8$th, $p+12$th, $p+16$th, $p+20$th settings ; let the

sum of one group of 36 readings be C_p. Four groups of 12 readings each, namely, the readings of the two unsymmetrical microscopes at the same settings; let the sums be denoted by C'_p ($p=1, 2, 3, 4$), then it is not difficult to see that

$$\Sigma_s C_{s.\,p-s} = C_p + C'_{p-1}$$
$$\Sigma_s C_{s.\,p} = C_p + C'_p$$

and the eight normal equations become:

$$8X_p + 6\Sigma y + 6Z_p + 2Z_{p-1} = C_p + C'_{p-1}$$
$$6X_p + 2X_{p+1} + 6\Sigma y + 8Z_p = C_p + C'_p$$

or eliminating the Z's:

$$12(2X_p - X_{p-1} - X_{p+1}) = 8C_p + 8C'_{p-1} - 6C_p - 6C'_p - 2C_{p-1} - 2C'_{p-1}$$
$$= 2C_p - 2C_{p-1} + 6C'_{p-1} - 6C_p$$

Solving this difference equation

$$6(X_p - X_{p-1}) = 3C'_{p-1} - C_{p-1}$$

plus an arbitrary constant, which is determined by the condition

$$\Sigma(X_p - X_{p-1}) = 0.$$

Hence the following precepts for computing are obtained:—

Form the four quantities $D_p = 3C'_p - C_p$ (to do this subtract the sum of the readings of the six symmetrical microscopes for each setting of the circle from three times the sum of the two unsymmetrical readings, and then add the 24 quantities thus obtained in four groups of six); then

$$6X_p = \tfrac{3}{8}(D_{p-1} - D_p) + \tfrac{1}{8}(D_{p+2} - D_{p+1})$$

and if the weight of a single observation be unity, the weight of the determination of $\frac{X_p}{6}$ is $\frac{144}{5} = 28{\cdot}8$.

On a modified form of Revolving Occulter for adapting the exposure of the Sun's Corona to its actinic intensity at all distances from the Moon's limb. By Professor David P. Todd, Director of Amherst College Observatory.

(*Communicated by the Secretaries.*)

Fifty years have now elapsed since the Sun's corona was first daguerreotyped. A heliometer with uncorrected objective was used, and it is not certain that the clockwork was sufficiently good to have prevented the blurring inseparable from the long exposure required by the relatively insensitive silver iodised surface. Still, the daguerreotype of the corona of 1851 compares very favourably with many photographs of the most recent

eclipses, although the latter are often taken with specially corrected objectives, practically perfect clock-motion, and relatively short exposure.

De la Rue, in the Spanish eclipse of 1860, made the next successful attempt, using wet collodion plates; and so fine were his photographs that they proved conclusively the connection of the corona with the Sun and not the Moon, because their detail was so sharp that the dark disc of the latter was shown passing over one filament after another; and subsequent measurement showed that these were stationary with reference to the Sun throughout totality.

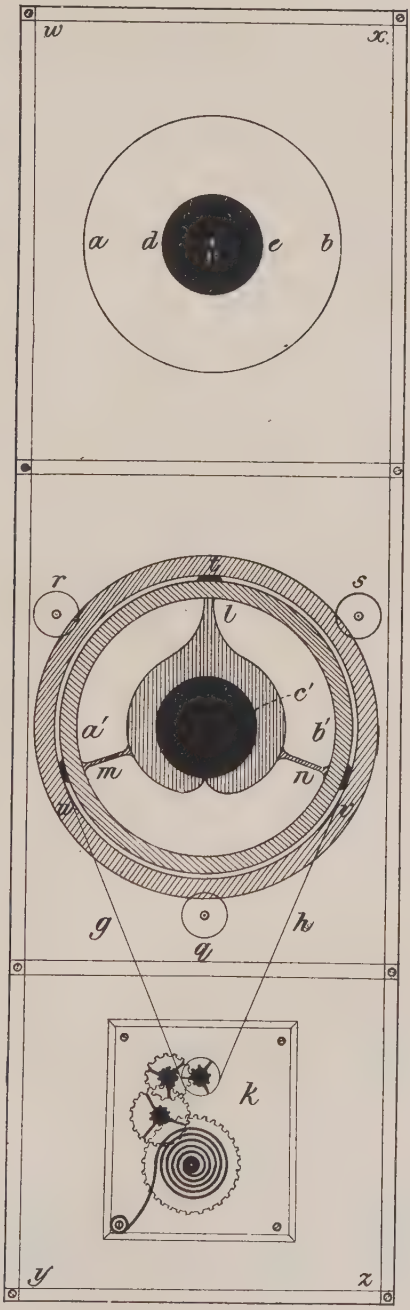
Lord Lindsay's Indian expedition in 1871 brought back perhaps the finest photographs of the corona ever taken—again wet plates, with excellent lenses and clockwork, and the expert manipulation of his photographer, Mr. Davis.

I think it an open question whether the old-fashioned wet-plate pictures of the corona will ever be surpassed by the modern and more convenient dry-plate ones, except by the adoption of some device, mechanical and automatic, for reducing the excessive actinic effect of the innermost corona, and the consequent halation obliterating all detail. To this end I constructed a device for photographing the corona in rings, described elsewhere;* but being satisfied that the scheme of Mr. Burckhalter is superior to it, I have re-designed and constructed a revolving occulter for the eclipse of 1901 May 18. It will be seen that I have overcome the most serious objection to the original form of the device—namely, the necessity for perforating the sensitive glass plate.

The illustration (Plate 16) readily shows how this is accomplished. A light frame, $w\,x\,y\,z$, slides in grooved ways about one inch inside of the focal plane. The circular field is $a\,b$, the size of the Moon $d\,e$; and when the occulter is not in use the frame is pulled out so that $d\,e$ is central over c. Photographs are here taken, as usual, so as to compare directly with those taken later by the same instrument, but modified by the occulter.

By pushing in the frame (up in the figure) to an accurate stop, which movement takes but half a second, c' takes the position in the field formerly occupied by c. The clockwork, k, is set going, and the exposure begins. By means of a light cord, $g\,h$, running round a pinion of k and the rim, $t\,u\,v$, the latter is revolved several turns per second. This rim is, of course, made very light, spun from thin aluminium, and exactly circular in all its sections. Its weight is but two ounces, and instead of turning on a central pivot, its outer edge slips through three accurately-adjusted friction wheels, $q\,r\,s$. The best disposition of these is shown in the illustration. One of them, q, should be as near as possible to k. The interior of this whirling ring has a groove, past which it is possible to slip, accurately and firmly, a black disc, $a'\,b'$, touching the rim at only three points. The black discs

* *Stars and Telescopes*, Sampson Low, Marston & Co., 1900, p. 463.


w
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are cut to any figure desired, from stiff corrugated strawboard packing, double faced. This is very strong; the finished disc or occulting screen weighs less than a half-ounce, and to change one disc for another occupies but four or five seconds.

The critical part of the apparatus is of course the heart-shaped figure within a' b', and attached to it at the points l, m, and n.

By carefully calculating the relative arc-values of exposure, central about c', it is easy to lay out a curve, point by point, and cut the corresponding disc, which, as the occulter whirls round, shall give any desired integral of exposure to the corona on a single plate, from $0^s{\cdot}1$ close to the Moon's limb up to 30^s at $30'$ distance.

The extraordinary length of totality in 1901 affords a ready opportunity for the substitution of several discs, calculated on different data as to the actinic intensity of the coronal light at various distances from the limb. There will be abundant time also for two or three exposures with each type of occulting disc. The size of the Moon's disc in the instrument I have adapted is $1^m{\cdot}8$—a 12-inch metal speculum of 15 feet focal length. For the photographic part I am depending upon the Standard Eastman films backed, as exhibiting less halative effect than glass plates. The mirror has a simple mounting moved by the glycerine clock, which affords that readiness of quick and accurate adjustment necessary to get the Moon's image exactly central round the c' of the occulter, and to maintain it there during the long totality.

What the new occulter is capable of doing I hope to be able to tell better after the eclipse is over. In rebuilding it I should make two further improvements: (*a*) to put the occulting disc within a few millimetres of the focal plane; (*b*) to rotate it by a clockwork whose rate is instantly changeable within wide limits, as I think the occulter should turn round not only at a perfectly uniform rate, but should make only one complete rotation during a single exposure, whatever its length. But these changes must be reserved for 1904.

Mean Areas and Heliographic Latitudes of Sun-spots in the year 1900, deduced from Photographs taken at the Royal Observatory, Greenwich, at Dehra Dûn (India) and in Mauritius.

(*Communicated by the Astronomer Royal.*)

The results here given are in continuation of those printed in the *Monthly Notices*, vol. lxi. p. 3, and are deduced from the measurements of photographs taken at the Royal Observatory, Greenwich; at Dehra Dûn, India; and at the Royal Alfred Observatory, Mauritius.

Table I. gives the mean daily areas of umbræ, whole spots, and faculæ for each synodic rotation of the Sun in 1900; and Table II. gives the same particulars for the entire year 1900 and the eleven preceding years for the sake of comparison. The areas are given in two forms: first, projected areas, that is to say, as millionths of the Sun's apparent disc; and, next, areas as corrected for foreshortening, the areas in this case being expressed in millionths of the Sun's visible hemisphere.

Table III. exhibits for each rotation in 1900 the mean daily area of whole spots, the mean heliographic latitude of the spotted area, and the mean distance from the equator of all spots; and Table IV. gives the same information for the year as a whole, similar results from 1889 to 1899 being added, as in the case of Table II. Tables II. and IV. are thus in continuation of the similar tables for the years 1874 to 1888 on pp. 381 and 382 of vol. xlix. of the *Monthly Notices*.

The rotations in Table I. and Table III. are numbered in continuation of Carrington's series (Observations of Solar Spots made at Redhill by R. C. Carrington, F.R.S.), No. 1, being the rotation commencing 1853 November 9. The assumed prime meridian is that which passed through the ascending node at mean noon on 1854 January 1, and the assumed period of the Sun's sidereal rotation is 25·38 days. The dates of the commencement of the rotations are given in Greenwich civil time, reckoning from mean midnight.

Table I.

No. of Rotation.	Date of Commencement of each Rotation.	No. of days on which Photographs were taken.	Mean of Daily Areas.					
			Projected.			Corrected for foreshortening.		
			Umbræ.	Whole Spots.	Faculæ.	Umbræ.	Whole Spots.	Faculæ.
	1899.							
618	Dec. 7·76	27	10	70	236	7	51	277
	1900.							
619	Jan. 4·09	27	16	99	161	10	63	182
620	Jan. 31·42	25	18	133	327	13	114	376
621	Feb. 27·77	26	31	155	215	26	136	242
622	Mar. 27·08	27	22	124	221	15	87	257
623	Apr. 23 35	28	50	253	312	30	154	388
624	May 20·58	26	19	91	241	14	68	296
625	June 16·78	27	52	163	104	51	134	152
626	July 13·99	27	16	58	126	10	39	137
627	Aug. 10 20	27	6	28	36	5	23	48
628	Sept. 6·45	27	8	21	95	5	13	120
629	Oct. 3·72	28	54	210	88	42	159	104
630	Oct. 31·01	27	5	18	66	3	12	77
631	Nov. 27·32	28	0	0	14	0	0	23

TABLE II.

Year.	No. of days on which Photographs were taken.	Mean of Daily Areas.					
		Projected.			Corrected for foreshortening.		
		Umbræ.	Whole Spots.	Faculæ.	Umbræ.	White Spots.	Faculæ.
1889	360	17·9	103	107	13·1	78·0	131
1890	361	21·3	133	273	15·5	99·4	304
1891	363	120	745	1322	86·2	569	1412
1892	362	255	1596	3230	186	1214	3270
1893	362	327	1983	2287	234	1464	2404
1894	364	317	1728	1666	231	1282	1877
1895	364	237	1330	2059	169	974	2278
1896	364	127	745	1243	90	543	1410
1897	364	122	695	977	88	514	1149
1898	363	93	532	767	64	375	891
1899	364	27	159	297	18	111	337
1900	360	22	101	150	17	75	180

TABLE III.

No. of Rotation.	Date of Commencement of each Rotation.	No. of Days on which Photographs were taken.	Spots north of the Equator.		Spots south of the Equator.		Mean Heliographic Latitude of Entire Spotted Area.	Mean Distance from Equator of all Spots.
			Mean of Daily Areas.	Mean Heliographic Latitude.	Mean of Daily Areas.	Mean Heliographic. Latitude		
	1899			°		°	°	°
618	Dec. 7·76	27	29	7·16	22	11·61	− 0·97	9·09
	1900.							
619	Jan. 4·09	27	56	7·84	7	11·32	+ 5·65	8·24
620	Jan. 31·42	25	105	9·48	10	9·63	+ 7·89	9·50
621	Feb. 27·77	26	2·5	4·38	134	11·99	− 11·69	11·85
622	Mar. 27·08	27	15	7·18	72	11·13	− 8·02	10·46
623	Apr. 23·35	28	24	4·35	130	7·57	− 5·57	7·08
624	May 20·58	26	56	3·83	11	8·06	+ 1·86	4·53
625	June 16·78	27	65	4·38	69	6·95	− 1·43	5·70
626	July 13·99	27	4	8·09	36	6·43	− 5·09	6·58
627	Aug. 10·20	27	14	3·49	9	13·13	− 3·29	7·43
628	Sept. 6·45	27	3	7·95	9	4·71	− 1·54	5·52
629	Oct. 3·72	28	0·0	...	159	5·56	− 5·56	5·56
630	Oct. 31·01	27	12	9·52	0·8	6·95	+ 8·49	9·36
631	Nov. 27·32	28	0·0	...	0	...	...	...

TABLE IV.

Year.	No. of Days on which Photographs were taken.	Spots north of the Equator. Mean of Daily Areas.	Spots north of the Equator. Mean Heliographic Latitude.	Spots south of the Equator. Mean Daily Areas.	Spots south of the Equator. Mean Heliographic Latitude.	Mean Heliographic Latitude of Entire Spotted Area.	Mean Distance from Equator of all Spots.
			°		°	°	°
1889	360	5·0	7·26	73·0	11·90	−10·68	11·61
1890	361	53·1	22·20	46·3	21·75	+ 1·73	21·99
1891	363	401	20·49	169	19·91	+ 8 52	20·31
1892	362	607	15 09	607	21·69	− 3·29	18·39
1893	360	517	14·91	941	14·26	− 3·93	14·49
1894	364	543	12 31	739	15·56	− 3·75	14·18
1895	364	565	14·26	409	12·54	+ 3·01	13·54
1896	364	203	13·60	340	14·77	− 4·15	14·33
1897	364	196	8·32	318	7·73	− 1·62	7 96
1898	363	110	9·82	266	10 77	− 4·75	10·49
1899	364	23	6 18	88	10·43	− 6·95	9·54
1900	360	26	6 61	49	8·34	− 3·12	7·74

The principal features of the record for 1900 are :—

1. The decline in areas of umbræ, whole spots and faculæ has been continued, the mean daily spotted area being about two-thirds, and the amount of faculæ about half the corresponding quantities for 1899.

2. The decrease in the area of the umbræ is slight.

3. The northern hemisphere has, as in 1899, been much the less active, giving only one-third of the total mean spotted area.

4. Only seven groups seen for at least ten days and with an average area exceeding 100 millionths of the hemisphere appeared during the year, viz. :—January 28–February 6, March 26–April 17, April 27–May 6, April 27–May 6, May 21–June 1, June 15–24, October 17–28. The second and fifth of these groups may be identified with groups seen in a previous rotation ; the third and fourth which form one large group with a group in a subsequent rotation.

5. There has been a further approach towards the equator in the mean latitude of spots.

6. Out of 360 days on which photographs were obtained there were 191 days without spots, 205 without faculæ, and 79 of these days were without spots or faculæ.

7. The minimum does not appear to have been reached in 1900 ; at the date of this paper (1901 July) no spot of high latitude marking the beginning of a new cycle has been seen.

1901 *July.*

Sun-spots and Magnetic Disturbance. By William Ellis, F.R.S.

The paper by Father Sidgreaves, appearing in vol. 54 of the *Memoirs of the Royal Astronomical Society*, "On the Connexion between Solar Spots and Earth-magnetic Storms," takes up a difficult subject. The general correspondence between the rise and fall of solar spots and terrestrial magnetism, whether measured by the variation of magnetic diurnal range or by the number of days of magnetic disturbance and storm, has been sufficiently well shown; but as regards correspondence in individual particulars, little that is really satisfactory has been so far evolved. The Rev. A. L. Cortie (*Monthly Notices* for 1900 May), in treating of the duration of Sun-spots, has shown that numerous groups are seen through several successive rotations of the Sun, but the author of the paper first mentioned has carried the matter further, by undertaking a considerable discussion of the question of the extent to which relation between individual solar spots and terrestrial magnetic disturbances and storms may be traced. Any serious consideration of this question deserves attention, since such work usually involves much labour. The general effect observable by those who have studied the matter is that, in our latitude, there may be at one time a large solar spot with great magnetic disturbance accompanied by remarkable aurora (as in 1882 November and in 1898 September), when Sun, Earth, and Earth's atmosphere are all involved; at another time a considerable solar spot may appear without accompaniment of unusual magnetic movement; and again magnetic disturbance may occur without any noteworthy spot.

Lord Kelvin, in his presidential address to the Royal Society in 1892, estimating the amount of work which must be done at the Sun to produce a terrestrial magnetic storm, considered the result obtained as absolutely conclusive against the supposition that terrestrial magnetic storms are due to magnetic or other action of the Sun, adding that it seems as if we may be forced to conclude that the supposed connection between magnetic storms and Sun spots is unreal, and the seeming agreement between the periods a mere coincidence. But to show that the Sun does not directly produce magnetic disturbance was not to prove that no relation existed, or that the agreement between the periods was accidental. The fact of general relation, however it is to be explained, is so far evident that theory must take account of it (*Proc. Royal Society*, vol. lxiii. p. 64). Lord Kelvin, in demonstrating the improbability of the existence of direct connection, may be understood to have had more in mind such a circumstance as the simultaneous observation by Carrington and Hodgson of an outburst on the Sun on 1859 September 1, corresponding in time with a magnetic movement shown on the photographic magnetic records, to which indeed he had referred in an earlier

portion of his address. Carrington, one of the observers of the solar movement, while considering the phenomena as deserving of notice, said that "he would not have it supposed that he even leans towards hastily connecting them." An occurrence so striking attracted attention, but unfortunately the narrative became repeated with exaggeration of statement, inducing a belief in direct connection. But time at last showed that here was apparently misconception, for although the Sun, in the ordinary routine of solar work, has since been unremittingly watched, and a continuous photographic magnetic record also maintained, similar conditions have not been again observed. The magnetic motion, in the case in question, was in itself in no way remarkable, was indeed slight, and of a character and magnitude such as often occurs, and much greater movements are also sufficiently numerous, but yet direct correspondence has not been made out. The apparent connection, thus having in after years received no further confirmation from observation, was shown also by Lord Kelvin thirty-three years afterwards to be from other considerations improbable. But, as before said, the general relation, both with variation of diurnal range and with frequency of magnetic disturbance and storm, is undoubted.

A later sentence in the same address runs: "We have at present two good and sure connections between magnetic storms and other phenomena: the aurora above, and the earth currents below, are certainly in full working sympathy with magnetic storms." The observation of earth currents at Greenwich Observatory, continued through many years, entirely supports this statement. When the ordinary diurnal magnetic variation is alone present, whether at maximum or minimum of Sun spots, earth currents are extremely feeble; but the appearance of magnetic disturbance, superposed on the ordinary diurnal movement, brings up at once a corresponding active earth current, and in very pronounced cases there may be also aurora. The magnetic irregularity and the active earth current are twin manifestations of an energy in the sense that one is never present without the other. Even a small superposed magnetic movement occurring suddenly is at once accompanied by a brisk little earth current. The correspondence is complete, the earth current being ever present with magnetic irregularity or storm. The same condition is found to exist at other places.

Thus in our latitude we may have (1) a large Sun-spot accompanied by magnetic disturbance + earth current, and, it may be, aurora—that is, the disturbing element involves both Sun and Earth; or (2) a considerable Sun-spot may appear with magnetic quiet; or (3) magnetic disturbance + earth current with solar quiet. In case 2 the Sun only is involved, and in case 3 the Earth only. Father Sidgreaves appears now to make out that the greater the Sun-spot the more frequently relatively is there accompaniment of magnetic disturbance or storm; not that there is direct correspondence, but that during a certain period there is

disturbance over a large field, influencing both Sun and Earth, the Sun-spot, or whatever produces it, being usually a determining element, not in all cases apparently, since a considerable spot may appear without magnetic storm ; and magnetic disturbance may arise with solar quiet—that is, the area of disturbance may be variable. Whatever may be the cause that extends or restricts the area, we obtain a knowledge of its extent unfortunately only by the Sun-spot on the one side, and by the magnetic disturbance and its accompaniments on the other side—phenomena both of which may be in part or degree of the nature of secondary effects.

Another circumstance deserves notice. Many magnetic storms commence with a very sharp movement of lesser or greater magnitude occurring simultaneously in all elements, declination, horizontal force, and vertical force, with accompanying earth current. This first movement may somewhat precede the magnetic disturbance or storm, or it may itself usher in the storm. In either case the distinctive characteristic is that the initial movement is sudden, sometimes sudden and large, and in the majority of cases at Greenwich is in the same direction, increasing the magnetic declination, horizontal force, and vertical force. And any marked or considerable initial movement becomes felt at the same absolute time—not nearly at the same time, but absolutely so, as nearly as the scale employed for photographic registration will allow of measurement—at places widely distributed over the earth's surface, each place having, as at Greenwich, its own distinctive characteristic as regards variation of the different magnetic elements. Magnetic disturbance having once set in, the following movements show similarity at places not far apart, but considerable dissimilarity at places widely separated ; the first impulse however coming commonly after some period of magnetic quiet is found, as described, to be in a special degree simultaneous at different places. The Earth as a whole seems to feel an instantaneous shock, which would thus appear to be by action from without. (*Proc. Royal Society*, vol. 52, p. 191.)

As to the fluctuations that occur in magnetic storms, Sir George Airy, in a paper "First Analysis of 177 Magnetic Storms," communicated to the Royal Society in 1863, suggested that there may be something in proximity to the earth which he would call a magnetic ether, in which currents exist liable to interruptions or perversions that produce violent eddies and whirls, instancing the cyclonic and other phenomena of atmospheric storms and whirlpools in water ; and Dr. Schmidt (*Terrestrial Magnetism*, vol. 5, p. 87), considering the diurnal magnetic variation to be referable to electric currents in the upper regions of the atmosphere, believes that the immediate cause of magnetic storms is to be referred to electric whirls or vortices which separate themselves from the general electric field in the atmosphere just as do the cyclones and anti-cyclones known to meteorologists, and he is led to conclude that for the greater part the

causes of our observed magnetic storms come from outside the earth's crust.

There is yet a phase of magnetic disturbance about which something may be said. Active magnetic disturbance and magnetic storms cluster principally, as we know, about periods of Sun-spot maximum, those about Sun-spot minimum being characterised by more or less prolonged magnetic quiet. But considering magnetic disturbance with reference to the year, it is found that at Greenwich there is also a seasonal variation. The number of days of magnetic disturbance during the period 1848 to 1897, classed in my paper (*Monthly Notices* for 1900 December) as being days of active and great disturbance, is 374, and their seasonal distribution, taking February, March, and April to represent spring, and May, June and July to represent summer, and so on, is as follows :—

Number of Days of Active and Great Magnetic Disturbances at Greenwich, 1848 to 1897.

Spring.	Summer.	Autumn.	Winter.
121	66	115	72
	Near equinoxes 236.		
	Near solstices 138.		

There is a certain irregularity in individual years, noteworthy disturbance appearing sometimes unusually early in the year, as in 1861 January, and sometimes unusually late, as in 1882 November ; but in the aggregate of years it is seen that a distinct seasonal variation exists. A closer examination shows that the spring maximum appears, on the whole, to fall somewhat before the equinox, and the autumn maximum somewhat after the equinox, in the months of February and October respectively. Thus we have, superposed on the variation of magnetic disturbance with Sun-spot frequency, a seasonal variation with maxima at the equinoxes and minima at the solstices, showing that the earth or its near surroundings has a part in determining the form in which the external influence reaches us. The related physical circumstance is that at the equinoxes, when disturbance is more frequent, the whole surface of the earth comes under the influence of the sun, whilst at the solstices, when magnetic disturbance is less frequent, a portion of the surface remains for a considerable period in shadow.

Attention may be drawn also to the relation existing between magnetic disturbance and the aurora. In our latitude not only do both phenomena vary with Sun-spot frequency, but the aurora is at Greenwich subject to a like seasonal variation. Extracting from Mr. R. C. Mossman's paper on "The Aurora Borealis in London from 1707 to 1895" (*Journal of the Scottish Meteorological Society* for 1897), the number of days of aurora during the years

1848 to 1895, and combining the months as for magnetic disturbance, we find :—

Number of Days of Aurora in London, 1848 to 1895.

Spring.	Summer.	Autumn.	Winter.
57	14	63	43
	Near equinoxes 120.		
	Near solstices 57.		

Here is a seasonal variation similar to that in number of days of magnetic disturbance, and here also the spring and autumn maxima occur in the months of February and October respectively. In higher latitudes the seasonal variation of the aurora is different, showing the local terrestrial effect to be different at different places on the Earth's surface ; is it also different in the case of number of days of magnetic disturbance ?

Observations of the Sun during 1901 May 17, 18, and 20, at Mells, near Frome, 10 miles due south of Bath. By Maures Horner.

On several occasions I have tried to examine the edge of the solar surface before and after a total eclipse in order to ascertain whether the prominences and metallic eruptions, which are seen by means of the spectroscope at other times compare in size and shape with the phenomena of a total eclipse.

In May last there was an opportunity of observing the Sun while the total eclipse was actually in progress, but owing to the screen of east wind haze—so persistent all through the spring—it was impossible to get satisfactory results, although the sky was remarkably free from cirrus and cumulus. On Friday, the 17th, I sketched the contour fairly accurately, using on the 5-inch Cooke one prism of very dense Iena glass by Hilger, which gives almost perfect definition, and widely separates the sodium lines with a low power. The drawing shows little solar activity, the E.N.E. limb alone displaying a certain amount of disturbance, probably due to the collection of small spots which came into view on the 19th. Further south, on the same side, was a fairly large prominence, with the peculiarity of a curious patch of bright colour between the two points. The same elevated bright cloud appearance was also visible in the south-west quadrant west of two rather dim forms at about 200°, which were quite conspicuous whenever the haze became a little more transparent. On the north-west limb there was a long attenuated form and a bright spot at the exact west point. With these exceptions the Sun's limb was practically undisturbed.

The haze was bad enough at noon on Friday, and therefore at 5.30 on Saturday morning there was not much probability of any

useful observation. However, I got up and carefully examined the most likely places to the east north-east and south south-west. At the former, near 83°, there were two brightish forms leaning southwards, whilst at 200° the double prominence of the day before had taken a more pyramidal shape of rather distinct outline. On the extreme north limb the surface was exactly like short soft hairs, quite different from the south, where the surface was smooth, with a long, attenuated filament pointing to two small spots rising from the surface. Later in the morning, when the haze had cleared off somewhat, I tried for the so-called coronal line No. 1474, which was at first quite easily observed. It was very plain just below the east point at 100°, also above at 60°, less so at 40°, and then again west of the north point there seemed to be another extension about 340°, and from there until 300° it was fairly strong, but invisible after that either on account of the haze or else from real solar causes.

These observations were very disappointing, and the more so because the Sun was free from cloud. However, on Monday, the 20th, a sensational phenomenon deserves some record. After observing the small group of spots above referred to, I found a feeble prominence and managed to show it, not without a good ideal of difficulty, to a friend who had never seen one before. Then, continuing round the limb, we reached the north part, and found a bright prominence of a curiously rounded dome-like form. This my friend saw without the smallest difficulty. As it was close on noon, I turned away to explain the sidereal clock and set watches. We certainly did not leave the telescope for more than five minutes, but on examining the limb again I found that the prominence had totally disappeared. I have seen many wonderful changes, but never such a sudden extinction of a bright but apparently not very active eruption. There could be no doubt about it, as the north part is much the most easily observed portion of the solar limb.

Solar Eclipse 1901 *May* 18. By W. Ernest Cooke, Government Astronomer.

Partial phase observed at the Perth Observatory, Western Australia.

			h	m	s	
First contact	1901 May	17,	17	13	26·64	G.M.T.
Last	„	„	18	54	06·10	„

Weather cloudy and showery, but Sun's limb was clear at both contacts, and observations considered good.

Perth Observatory, W.A.:
1901 *May* 18.

Estimations of Magnitude of Nova Aurigæ in 1899–1900, *with the mean results for the years* 1892–1900, *from observations at the Radcliffe Observatory, Oxford.*

(*Communicated by Arthur A. Rambaut, M.A., Sc.D., F.R.S., Radcliffe Observer.*)

The observations of *Nova Aurigæ* given in this paper are in continuation of those published in the *Monthly Notices*, vol. lix. p. 258. Estimations of the magnitude of this star have also appeared in vol. lii. pp. 430, 431 ; vol. liii. pp. 33, 34, 126 ; vol. lv. p. 164 ; vol. lvi. p. 234 ; and vol. lviii. p. 180.

A chart giving the approximate positions of the *Nova* and comparison stars will be found in vol. lii. p. 431. The letters denoting the comparison stars in this paper refer to this chart.

The estimations were made with the Barclay Equatorial of 10 inches aperture.

1899 April 1, $9\frac{1}{2}^{h}$.—*Nova* > $d=e=\xi$. Stars seen with difficulty ; field bright with scattered light ; slight condensation found on object-glass. Powers used, 80 and 100. (C.)

1899 April 10, 10^{h}.—*Nova* slightly brighter than ξ, d, and e by about 0·3 or 0·4 magnitude. Sky suddenly clouded over immediately after this observation. (R.)

1900 October 4, $10\frac{3}{4}^{h}$.—*Nova* fainter than any of the comparison stars of the chart except p, which it excelled in brightness by 0·3 magnitude. *Nova* seemed nebulous, with occasionally a stellar nucleus. Bright moonlight. With power 250 the following estimations of magnitude of *Nova* and comparison stars were made : $a=10{\cdot}5$; $b=11{\cdot}5$; $c=11{\cdot}7$; $\xi=12{\cdot}7$; $d=12{\cdot}7$; $e=13{\cdot}0$; $f=13{\cdot}2$; *Nova*$=13{\cdot}4$; $p=13{\cdot}7$. (R.)

1900 November 17, 10^{h}.—*Nova* seems to be about 0·2 or 0·3 brighter than on October 4. With power 250, the following estimations of magnitude of *Nova* and comparison stars were made : $a=10{\cdot}5$; $b=11{\cdot}5$; $c=11{\cdot}6$; d and $\xi=12{\cdot}6$; $e=13{\cdot}0$; *Nova*$=13{\cdot}2$; $f=13{\cdot}4$; $p=13{\cdot}7$. There is a wide companion to p nearly same magnitude (about 13·9), and this companion appears to be elongated. It is situated between a and p at about $\frac{1}{4}$ the distance from a to p. [Subsequently identified with M of Burnham's chart, *Monthly Notices*, vol. lii. facing p. 436.] (R.)

1900 December 13, 7^{h}.—*Nova* is fainter than any of the comparison stars in chart except f and p. *Nova* estimated as $=f$ and $>p$. (R.)

Adopting for the comparison stars the magnitudes given in vol. lii. p. 431, we have for the magnitude of *Nova Aurigæ* :

1899	h		
April	1, $9\frac{1}{2}$	13·5	Observer C.
	10, 10	13·3	„ R.
1900	h		
Oct.	4, $10\frac{3}{4}$	14·1	„ R.
Nov.	17, 10	13·9	„ R.
Dec.	13, 7	13·8	„ R.

Observers : R. Mr. Robinson.
C. Mr. McClellan.

The mean magnitudes from 1892 to 1900 are :

1892	4·5 to < 14·0	Feb. 3 to March 31
„	9·5 to 9·8	Sept. 8 to end of year
1893	9·7	
1894	9·7	
1895	9·7	
1896	...	
1897	11·4	
1898	12·0	
1899	13·4	
1900	13·9	

Radcliffe Observatory, Oxford:
1901 *June* 13.

Further Observations of the New Star in Perseus made at the Radcliffe Observatory, Oxford.

(*Communicated by Arthur A. Rambaut, M.A., Sc.D., F.R.S., Radcliffe Observer.*)

This paper contains the results of observations upon the brightness and colour of the new star in *Perseus* made at the Radcliffe Observatory since the date of the last meeting of the Society, and is in continuation of the Notes, on the same subject, already communicated to the Society and published in the *Monthly Notices* for March, April, and May.

With the exception of two observations made by Mr. Robinson on May 12 and 14, all the estimations were made with

telescopic aid. After May 18 the star could no longer be followed with the Barclay Equatorial, and accordingly from that date most of the observations were made with the $7\frac{1}{2}$-inch Heliometer.

The observations were for the most part difficult owing to the amount of twilight and the low altitude of the star. Between May 11 and 24 the sky was generally very clear, and an almost continuous series of observations was obtained.

The observations this month continue to indicate the relationship already pointed out between the colour and magnitude of the star, on the whole the redness of the star increasing as the brightness is diminished.

The light period continues to show considerable fluctuations. Between May 11 and 24 there are two periods of four or five days followed by a three days' period.

The amplitude of the oscillation in brightness appears to be about $1\frac{1}{2}$ magnitudes, as before; while the mean magnitude about which it oscillates has remained unaltered since April 20.

TABLE I.

List of Stars used for Comparison with Nova Persei.

Ref. No.	Name of Star.	Harvard Photom. Mag.	Ref. No.	Name of Star.	Harvard Photom. Mag.
40	κ Persei	3·95	53	36 Persei	5·40
42	ν Persei	4·00	54	Arg. Z. +44°, 734	6·04
48	σ Persei	4·39	55	Arg. Z. +44°, 648	6·47
50	l Persei	4·84	56	Arg. Z. +43°, 818	5·97
52	30 Persei	5·37	59	Arg. Z. +42°, 795	6·30*

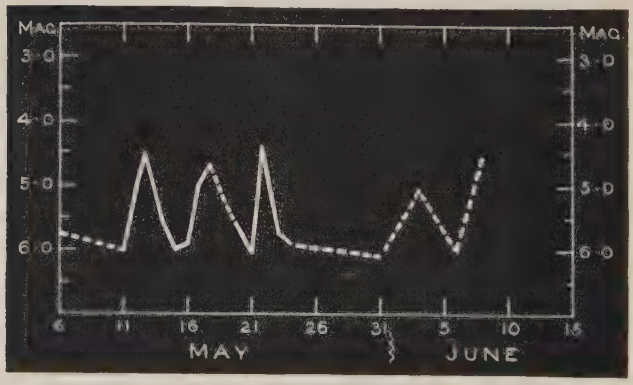
* Harvard Photom. D.M.

TABLE II.

Means of Estimations of Magnitude of Nova Persei.

G.M.T. 1901.	h	m	Observer.	Reference Stars.	Mean Mag.	Adopted Mag. for the Night.
May 11	9	0	R.	56	6·10	6·05
	9	5	C.	56	6·00	
12	9	53	W.	52, 50, 55, 56, 53	5·36	5·20
	10	0	C.	50, 52	5·05	
	10	5	R.	50	5·00	
13	8	35	R.	50, 42	4·40	4·50
	10	0	W.	50, 48, 40	4·60	
14	9	53	C.	52, 56	5·55	5·55
	10	0	R.	50	(5·1)	
15	8	30	R.	52, 56	6 00	6·00

G.M.T. 1901.	h	m	Observer.	Reference Stars.	Mean Mag.	Adopted Mag. for the Night.
May 16	10	0	C.	52, 56	5·90	5·90
	10	15	R.	56, 52	5·90	
17	10	15	R.	52, 56	5·00	5·00
18	8	20	R.	42	4·80	4·65
	10	0	C.	50	4·50	
20	9	45	C.	52, 56	5·70	5·67
	9	47	R.	52, 56	5·60	
	9	47	R.	52, 56	5·70	
21	9	53	C.	52, 56	6·05	6·05
22	11	5	R.	42, 50, 40	4·40	4·40
23	9	45	R.	52, 56	5·70	5·70
24	9	45	R.	52, 56, 54, 59	5·88	5·88
31	9	45	R.	54, 56	6·10	6·10
June 3	11	0	R.	50, 52	5·05	5·05
6	10	30	R.	54, 56	6·00	6·00
8	11	0	R.	52, 50, 42, 40	4·57	4·57



Magnitudes of Nova Persei.

Observers' Notes on the Estimations of Magnitude.

1901.
May 12. No. 52 not visible to the naked eye, except perhaps momentarily. Sky very clear, but objects low. Twilight rather strong. (R.)

May 13. Very difficult; strong twilight. (R.) Half-weight to No. 48; this star is much higher than Nova. (W.) Could not see the Nova with the naked eye at 10^h; Nos. 40 and 42 were only occasionally visible through haze. (R.)

1901.
May 14. Observed with Marlborough, power 80. Nos. 50 and 52 were also easily visible in the "finder," but the Nova was only seen with some difficulty. Sky clear. (C.) Suspected a glimpse of the Nova with the naked eye at 10^h, when I estimated it to be fainter than No. 50 by 0·3. This observation is doubtful, but the Nova was certainly not brighter than magnitude 5. Sky very clear. (R.)
May 16. Observed with Marlborough, power 80. In "finder" Nova and No. 56 were equally difficult to pick up, but Nos. 52 and 50 were easily seen. (C.)
May 17. Sky hazy and cloudy; observations very difficult.
May 18. Twilight strong, and haze prevalent low down. Observation only approximate. (R.) Sky hazy; very difficult estimation. (C.)
May 20. Sky clear. (C.) The first observation by R. was taken with the Heliometer 7·5-inch; the second with the 1·83-inch "finder."
May 21. Sky very clear; many small stars visible near.
May 22. Observed near lower culmination. Altitude 5°.
May 23. In the 7·5-inch (Heliometer) the image of the Nova was equal in size, but inferior in brightness to No. 52. Sky very clear.
May 24. Sky very clear. A star (Arg. Z. + 42°, 803) follows No. 59 by 1^m, and is south. It is fainter than No. 59 by 0·5 mag.
May 31. Flickering images.
June 3. Images good at times; objects low.
June 8. Observed near meridian (sub polo). No. 40 is more than a magnitude brighter than 42, which is 3° lower in the haze. The magnitude estimations are sensibly confirmed by observer C. with the same telescope.

Observers' Remarks on Colour of Nova and Comparison Stars.

1901.
May 11. With the 10-inch Barclay and power 45 Nova is orange with very red fringe. (R.) Very red in 10-inch. Sky very hazy. Atmospheric conditions similar to those at time of sunset, when Sun was of a deep orange tint. (C.)
May 12. Exceptionally clear. Nova's colour in the $1\frac{3}{4}$-inch is a much fainter red than I have seen for more than a month. The stars were very low, and No. 14 exhibited rapidly changing colours of the spectrum. Images generally very good. (W.)
May 13. Sky is not nearly so clear as last night; there is an absence of red tint from Nova; I could detect none in the $1\frac{3}{4}$-inch. The observations are a little doubtful owing to small altitude. (W.) Nova in the Barclay has a large central yellow disc with small but bright red wings. No. 42 is yellowish this evening. (R.)
May 15. Redness of Nova in the Barclay becomes more apparent as the twilight wanes.
May 16. Nova very red in Marlborough. (C.) Nova red in Marlborough. (R.)
May 20. Nova very red in Marlborough. (C.) Contrast in colours of Nova and comparison stars with the Heliometer. Nova red. (R.)
May 21. Nova red in Marlborough.
May 22. Nova red in Heliometer, and No. 40 reddish-yellow. Objects low.
May 23 and 24. Nova red in Heliometer.
June 6. Nova not very red in Heliometer. Objects low.
June 8. Nova and No. 40 red in Heliometer. Low in haze.

The observers were:

Mr. Wickham, indicated by W.
Mr. Robinson, „ „ R.
Mr. McClellan, „ „ C.

Radcliffe Observatory, Oxford:
1901 *June* 13.

Observations of Nova Persei made at Birr Castle Observatory, Parsonstown. By the Earl of Rosse, F.R.S.

During the time that *Nova Persei* was visible every opportunity was taken for observing that interesting object, but I happened to be in England at the time and no authentic information as to its existence reached us until after its maximum brightness had been much reduced, and subsequently the extreme uncertainty of weather which prevailed at that time much impeded observations. For that reason, however, the work of others was also fragmentary, and in the hope of filling gaps rather than of adding any important matter the notes made at the time from observations principally by Dr. Boeddicker are sent in. Spectroscopic observations were not satisfactory, the instruments having lain unused for some years and having deteriorated with time.

With a direct-vision prism, however, placed between the eye and the eyepiece of the 3-foot reflector, the red line was quite unmistakable. Others in the more refrangible regions were at times satisfactorily seen, but it was not always easy with the means at our disposal to distinguish between a bright line and an absence of dark lines in a continuous spectrum.

Through the whole time that the star was visible the prevailing easterly wind and the accompanying thin clouds generally diffused the star's light and destroyed definition of prism used without a slit. No photography was attempted.

1901 March 1, 10^h M. T. Greenwich. A hurried estimate between clouds. *a Persei* $\geq$ *Nova*.

1901 March 2, 10^h to $10^h\ 50^m$. Sky bad. Passing clouds and haze. Examination during bright interval. *a Persei* > *Nova*. *Nova* distinctly red. With direct-vision spectroscope before eyepiece, the red very intense at its extreme end, cut off from yellow by very decided dark bands or lines. Further dark lines, very conspicuous, in blue, also—but fainter, more suspected—in violet.

1901 March 3. Passing clouds. *a Persei* > *Nova*; no perceptible change in magnitude since last night. Spectrum magnificently seen with cylindrical lens. Very bright line in red, separated from yellow by a broad, dark band (darkest towards red). Faint narrow dark line in orange, also one between yellow and green. Green fairly uniform, very intense and well defined towards blue, where it is abruptly cut off by a broad dark band. This is followed by a sharply defined bright band (or are there two?) in blue. Very dark band between

blue and violet. A bright band at times suspected in extreme violet.

1901 March 6. Storm and rain. One glimpse of *Nova*. Magnitude 3.

1901 March 7. Fairly clear. Well seen. Magnitude below 3. Extreme red very intense, terminated by a dark band towards yellow. Bright line in yellow. Dark bands in bluish green and blue. A brighter band in blue towards violet. Dark lines in violet suspected.

1901 March 8. Magnitude 4. Intensely red line followed by very dark absorption band. Dark band in green followed by a bright band. Very dark bands in blue. Bright band in blue towards violet (or are there dark absorption bands in violet towards the blue ?). Violet seems of uneven brightness ; there is decidedly a brighter part or band in its extreme end. Dark bands or lines (in violet) suspected.

1901 March 13. Sky very bad. Hazy. Magnitude below 4. Spectrum very badly seen owing to haze and thin clouds.

1901 March 14, $9^h\ 40^m$. Sky very hazy, stars very dim. *Nova* nearly $=o$, certainly $>\nu$. Magnitude increased to about 3·3. Extremely bright line in red followed by dark absorption bands. Dark lines in green, leaving central portion bright, looking like a bright band. Very conspicuous bright line in blue-green, also faint indication of a bright band in violet.

1901 March 21. Sky hazy. Images bad and unsteady. *Nova* fainter than ν but slightly brighter than κ. Red line very conspicuous, followed by very dark absorption band up to yellow. Dark lines suspected in green. Bright band in bluish green very striking, bright band in extreme violet unmistakable.

1901 March 22. Sky hazy. Images unsteady. 5th magnitude stars only visible with great difficulty. *Nova* below σ or 4·5 mag. ; almost 5.

1901 March 25. Clear, cold. ☾. *Nova* almost 6th mag.

1901 April 8. Sky very bad. Covered with cirro-s. Cleared somewhat at $11^h\ 40^m$; when low near the horizon the *Nova* became visible, having increased again to about 4·3 mag., *i.e.* ·2 or ·3 mag. below κ. Not to be reached with telescope.

1901 April 9. Clear for a short time. *Nova* out of reach with 3-foot reflector ; viewed with 4-inch refractor. Magnitude about 4·5, perhaps somewhat brighter. Slightly below κ, yet it

appeared as if this inferiority was due to the difference of colour. At times both equally well seen. Spectrum very striking and hardly, if at all, different since former observation. Very strongly pronounced red line. Bright band suspected in yellow. One, perhaps two, seen in green.

1901 April 10. Sky partly clear. *Nova* much decreased in brightness, below 32 *Persei*. Just on the verge of visibility with naked eye ; only to be seen in glimpses.

1901 April 11. Partly clear at first, then cloudy. *Nova* not very much fainter than last night. Still visible in glimpses, yet with less certainty. Magnitude decidedly below 6.

Further Observations of the New Star in Perseus (4).

By A. Stanley Williams.

During the month of May the position of *Nova Persei* was very unfavourable, and the observations were necessarily made with the star at a low altitude and on a bright sky. Notwithstanding, however, these unfavourable conditions, the estimates of the brightness of the *Nova* have been mostly quite satisfactory owing to fine weather and the generally clear sky even at a very low altitude.

The following estimates of the brightness of the new star were all made with a power of 35 on the $2\frac{3}{4}$-inch refractor ; and in no case, it may be mentioned, was there any uncertainty with regard to the identity of the object or of the comparison stars. No correction for difference of atmospheric absorption has been applied to the provisional magnitudes in the last column, although this correction would probably be quite sensible in the case of the evening observations, at any rate as regards the comparisons with 36 *Persei*. It is probable, therefore, that the provisional magnitudes from the evening observations may be slightly too low, though the influence of the peculiar colour of the star in possibly modifying the effects of absorption perhaps requires to be taken into consideration. In the morning observations the *Nova* and the two comparison stars 36 *Persei* and *a* (B.D.+44°, 734) were all nearly at the same altitude, and should have been about equally affected by absorption. In some cases, where comparisons were made in the evening at short intervals, the separate results are given, as they may be of assistance with regard to this question of absorption. The progressive diminution in brightness shown by the last three observations of May 22 may well have been due to the effect of increased absorption as the star approached the horizon. On May 23 we have a similar change.

Date. 1901.	G.M.T. h m	Observations.	Provisional Magnitude.
May 12	9 50	Nova = $a-2$ ($b-12$)	6·3
13	9 19	36 Persei + 0·3, $a-11$	5·4
14	9 21	36 Persei + 8, $a-2·7$	6·2
15	9 30	$a+1$ ($b-9$)	6·6
17	9 10	36 Persei + 4, $a-7$	5·8
	9 45	36 Persei + 5, $a-6$	5·9
18	9 30	36 Persei + 4, $a-7$	5·8
	9 50	36 Persei + 5, $a-6$	5·9
20	9 10	36 Persei + 8, $a-3$	6·2
	9 25	36 Persei + 9, $a-2$, $b-9$	6·2
	9 35	36 Persei + 11, $=a$, $b-9$	6·3
21	9 15	36 Persei + 9, $a-2$	6·3
	9 25	36 Persei + 10, $a-2$, $b-9$	6·2
	9 30	36 Persei + 11, $a-1$, $b-9$	6·3
	9 40	36 Persei + 10, $=a$, $b-10$	6·3
22	9 20	36 Persei + 1, $a-11$	5·45
	9 35	36 Persei − 5	4·9
	9 40	36 Persei − 4	5·0
	9 45	36 Persei − 2	5·2
23	9 20	36 Persei + 6, $a-5$	6·0
	9 36	36 Persei + 9, $a-3$	6·25
	9 40	36 Persei + 9, $a-2$	6·3
24	9 30	$a-5$	6·0
	9 35	36 Persei + 8, $a-3$	6·2
	9 36	36 Persei + 8, $a-3$	6·2
	9 38	36 Persei + 8, $a-3$	6·2
June 6	13 50	36 Persei + 8·7, $a-5·7$	6·1
8	13 46	36 Persei − 1 ($a-15$)	5·3
10	14 19	36 Persei + 12·5, $a-2$	6·45

Notes.

May 12, very clear. May 13, very clear; mean of 3 observations between $9^h\ 8^m$ and $9^h\ 30^m$. May 14, exceedingly clear; mean of 3 observations between $9^h\ 10^m$ and $9^h\ 35^m$. May 15, not very clear. May 17, clear intervals between clouds. May 18, a little hazy. May 20 and 21, very clear. May 22, very clear; star too bright for proper comparison with a. May 23, very clear. May 24, considerable thin cirrus cloud about, but a narrow strip of sky just above the horizon seemed perfectly clear, and it is believed the estimates were not affected by the cloud. June 6, hazy; mean of 3 observations between $13^h\ 40^m$ and $14^h\ 0^m$. June 8, very clear; mean of 3 observations between $13^h\ 5^m$ and $14^h\ 17^m$. June 10, considerable thin cloud, but the observations were certainly not affected by this; mean of 4 sets of comparisons between $14^h\ 7^m$ and $14^h\ 30^m$. The star designated b is B.D. + 43°, 730.

Although the fluctuations in brightness have continued, the star does not seem to have varied to the same extent as formerly. The observations show that the star was more or less bright on the following dates: May 13, 17–18 (slight), 22, and June 8. Apart from these temporary or periodical fluctuations, the *Nova* seems to have remained almost stationary during the past month.

Observations of colour have been practically impossible lately owing to the low altitude, but on May 13 the star seemed strongly reddish, and on May 14 it was noted as being evidently a very deep red. On May 22, when the star was bright, it appeared as a flaming bright star of a pronounced red colour, even at $9^h\ 45^m$, when the altitude was very low. On June 8, at $13^h\ 55^m$, the colour seemed to be orange.

1901 June 12.

Secular Variation in the Period of R Carinæ.

By Alexander W. Roberts, D.Sc.

The periods of several of the Southern Long Period Variables seem subject to secular variation, but with the exception of *R Carinæ* the observations are either too discontinuous or too meagre to afford even a remote idea of the amount and nature of the variation.

With regard to *R Carinæ* sufficient observations have, I think, accumulated during the past twenty years to indicate the salient characteristics of the irregularities which affect its variation.

The first observation of which we have any record is that made by Lacaille on 1752 March 3. As Lacaille estimated it as of the seventh magnitude, it could not then have been far from its maximum brightness. This observation is therefore of importance as fixing within certain limits the period of t' variable.

In 1867 two meridian observations of *R Carinæ* were made very near a maximum phase by Ellery at Melbourne.

The two observations are :—

1867 March 31	... ...	Magnitude 6·5
„ April 3	... ...	„ 6·0

This, taking the average rate and extent of variation of *R Carinæ*, would mean a maximum on or about 1867 April 25.

The observations made by Brisbane and Stone are not of value in any investigation dealing with the light changes of *R Carinæ*, as position rather than brightness was the purpose of their observations. It is indeed possible that Stone simply copied Lacaille's magnitude into his observation book.

The discovery of the variation of *R Carinæ* was made by Gould in 1871, and during this and the following three years continuous observations were made of it at Cordoba.

These yielded (*Uranometria Argentina*, p. 251) the following four maxima dates :—

1871 July 17	...	...	Julian Day 2404626
1872 June 10	...	...	„ 2404955
1873 April 12	...	...	„ 2405261
1874 March 4	...	...	„ 2405587

The long and highly valuable series of observations made by Tebbutt, at Windsor, New South Wales, was begun in 1880. Without these earlier observations by this indefatigable worker, an investigation of the secular inequalities of *R Carinæ* would not, I think, be possible at the present date.

In 1891 regular observations were begun at Lovedale, and have been carried on until now with only one short interval. We have accordingly continuous observations of *R Carinæ* for over twenty years. If we include Gould's observations the series extends, with one break, over thirty years.

The following list gives the various maxima which have been determined, beginning with the estimated maximum of 1867 (Melbourne).

Rotation No.	Maximum.	Julian Day.	Observed at.
1	1867 April 25	2403082	Melbourne
2	1871 July 17	2404626	Cordoba
3	1872 June 10	2404955	„
4	1873 April 12	2405261	„
5	1874 March 4	2405587	„
6	1880 Dec. 16	2408066	Windsor
7	1881 Oct. 21	2408375	„
8	1882 Aug. 30	2408688	„
9	1883 July 6	2408998	„
10	1884 May 24	2409321	„
11	1885 April 5	2409637	„
12	1886 Jan. 27	2409934	„
13	— Dec. 8	2410249	„
14	1887 Oct. 13	2410558	„
15	1889 June 23	2411177	„
16	1890 April 13	2411471	„
17	1892 Jan. 1	2412099	Lovedale
18	— Nov. 3	2412406	„
19	1893 Aug. 25	2412701	„
20	1894 July 2	2413012	„
21	1895 May 11	2413325	„
22	1898 Sept. 15	2414548	„
23	1899 July 12	2414848	„
24	1900 May 17	2415157	„
25	1901 March 20	2415465	„

As already stated, the observation made by Lacaille is of importance as indicating between what limits the mean value of the period of *R Carinæ* must lie. Lacaille's observation was

made on 1752 March 3, the magnitude on that date being 7·0. Now either *R Carinæ* was then rising to its maximum brightness or it had already passed it. An examination of the mean light curve indicates that *R Carinæ* passes what is apparently Lacaille's seventh magnitude standard 50 days before maximum or about 80 days after it. That is, in 1752 the maximum of *R Carinæ* took place either about January 23 or about May 22. The former date when related to recent maxima yields a period of 309·7 days, and the latter date 309·0 days. Between these two values the mean period of *R Carinæ* must lie.

In a recent catalogue of Southern Variable Stars (*Astronomical Journal*, vol. xxi. p. 85) I considered the former of the two values, 309·7 days, as the one which most closely corresponds with observation. With this period as a known quantity, the full elements of variation are :—

(I.) $M = 2415180 + 309^d{\cdot}7\,E + 25^d \cos(10° E - 180°)$

That is, a full cycle of the inequalities affecting the period of *R Carinæ* is completed in about thirty-one years.

With these elements the maximum dates given in column 4 have been computed. Column 5 gives the residuals between the dates so computed and the observed dates (column 3).

Rotation No.	E	Observed Max. J.D.	I. Computed Max. J.D.	O−C	II. Computed Max. J.D.	O−C
				d		d
1	−39	2403082	2403080·1	+ 1·9	2403086·5	− 4·5
2	34	2404626	2404626·8	− 0·8	2404636·5	−10·5
3	33	2404955	2404938·3	+16·7	2404947·7	+ 7·3
4	32	2405261	2405250·4	+10·6	2405259·3	+ 1·7
5	31	2405587	2405563·2	+23·8	2405571·3	+15·7
6	23	2408066	2408073·0	− 7·0	2408070·5	− 4·5
7	22	2408375	2408385·8	−10·8	2408382·5	− 7·5
8	21	2408688	2408697·9	− 9·9	2408694·1	− 6·1
9	20	2408998	2409009·4	−11·4	2409005·3	− 7·3
10	19	2409321	2409320·3	+ 0·7	2409316·2	+ 4·8
11	18	2409637	2409630·4	+ 6·6	2409626·7	+10·3
12	17	2409934	2409939·7	− 5·7	2409936·7	− 2·7
13	16	2410249	2410248·2	+ 0·8	2410246·2	+ 2·8
14	15	2410558	2410556·1	+ 1·9	2410555·3	+ 2·7
15	13	2411177	2411170·0	+ 7·0	2411172·0	+ 5·0
16	12	2411471	2411476·1	− 5·1	2411479·7	− 8·7
17	10	2412099	2412087·3	+11·7	2412094·1	+ 4·9
18	9	2412406	2412392·7	+13·3	2412400·7	+ 5·3
19	8	2412701	2412698·7	+ 2·3	2412707·1	− 6·1
20	7	2413012	2413003·5	+ 8·5	2413013·4	− 1·4
21	6	2413325	2413309·3	+15·7	2413319·5	+ 5·5
22	2	2414548	2414537·2	+10·8	2414543·9	+ 4·1
23	1	2414848	2414845·7	+ 2·3	2414750·3	− 2·3
24	0	2415157	2415155·0	+ 2·0	2415156·9	+ 0·1
25	+1	2415465	2415465·1	− 0·1	2415463·9	+ 1·1

To determine if any correction to the elements of variation :—

$$(I.)\quad M=2415180+309^{d}\cdot7\,E+25^{d}\cos(10^{\circ}\,E-180^{\circ})$$

would better satisfy the recorded observations, differential equations were formed from the above elements, and these being solved gave a system of corrections. Once more with the corrected elements a new set of differential equations was formed, and these gave as a final result :—

$$(II.)\quad M=2415172+309^{d}\cdot3\,E+23^{d}\cos(8^{\circ}\cdot2\,E-229\cdot1^{\circ})$$

The dates of maximum phase computed from these elements are given in column 6 of the preceding table, and the residuals in column 7.

The average residual computed from elements I. is

7·5 days ;

and the average residual computed from elements II.

5·3 days.

In order to exhibit the systematic variation in the duration of the period of *R Carinæ* in a more evident form, Plate 17 represents the discordances between observed and computed dates when the systematic correction

$$23^{d}\cos(8^{\circ}\cdot2\,E-229^{\circ}\cdot1)$$

is left out of account. The dotted curve in the figure is this systematic correction.

In this connection, however, it may be as well, in case of misapprehension, to point out that we have no certain assurance, and cannot have any for many years to come, that an expression of the form

$$a\cos(\theta E-M)$$

describes the secular irregularities affecting the period of *R Carinæ*.

It is assumed that the irregularities are of this nature, but until a full cycle is completed the assumption must have the character of an empirical explanation.

If we select a period of 309·7 days, one of the limits, it will be found that the residuals, when graphically represented, may be expressed under the form

$$a\cos(\theta E-M)+\beta\cos(2\theta E-N)\ ;$$

and this curve will as readily satisfy the discordances as the former.

Every year is, however, making a definite solution more possible, and in ten more years there will be sufficient data to

determine rigorously the amplitude and duration of the cycle of inequalities.

Meantime the curve given in Plate 17, and the elements set forth in Equation II., may be taken as generally descriptive of these inequalities, and any further and more refined consideration of fuller material will not materially alter the conclusions come to.

Put generally the conclusions are as follows :—

(1) The mean period of *R Carinæ* is 309·3 days, but this value varies from 305·8 days as one limit, to 312·8 days as the maximum limit.

(2) The lower limit was passed in 1896, and the maximum limit in 1877.

(3) At present (1901) the duration of the period is 307·4 days and is increasing.

(4) A full cycle of periodic inequalities is completed in 37 or 38 years.

One is tempted to wander into speculation as to the cause, or causes, of this anomaly ; but until we know more about the conditions of motion, or about the chemical changes, or about both combined, that produce the type of long period variation that we find in stars of the same class as *R Carinæ*, such excursions are unprofitable.

I think, however, the solution of the problem would be advanced materially if at each recurring maximum careful measures were made of the motion in the line of sight of the brighter variables. It could then be determined if this long period inequality is in any way connected with orbital movement.

Lovedale, South Africa :
1901 *May* 9.

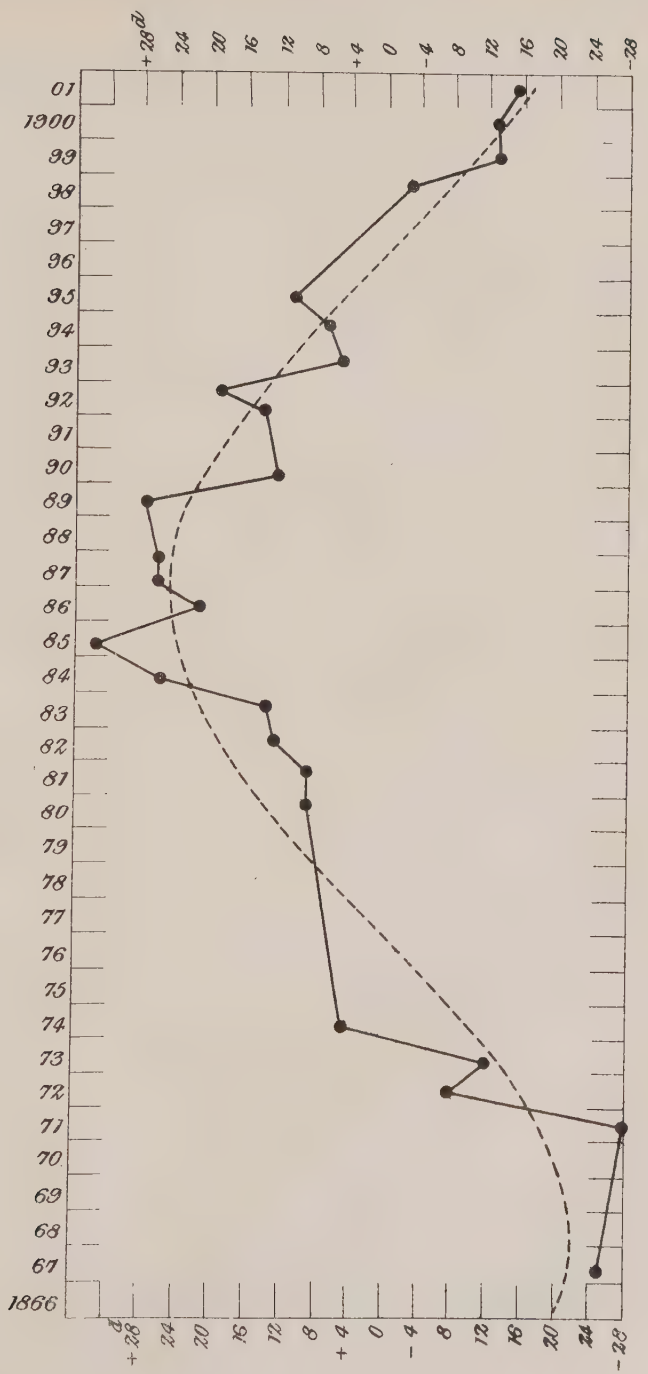
Measures of Double Stars made at Mr. Edward Crossley's Observatory, Bermerside, Halifax. By Joseph Gledhill.

The following measures of a selected list of important binary stars have been made with the Cooke Equatorial and Simms' Micrometer. Up to 1896 the object-glass was the $9\frac{1}{3}$-inch Cooke ; since that year a 9-inch photo-visual object-glass by the same makers has been used. The power most used was 290 ; occasionally 480 was used with advantage.

The usual method of measurement with a parallel-wire micrometer was followed.

Distances are almost always difficult to measure here owing to the unsteady air ; hence it is that most of the distances under 1″ recorded below are rather careful estimations than measures. Very few years afford half a dozen nights good enough for measures of distances under 1″.

The R.A. and dec. are for 1900. When two or more sets of measures have been made in one year the mean value is given.


Secular inequality in the period of R Carinae.
+28d
24
20
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12
8
4
0
-4
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-28
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1900
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69
68
67
1866

Name.	R.A. h m	Dec. ° ′	Date.	Position Angle. °	Distance. ″	Nights.
Σ 3062 ...	0 1	+ 57 53	1895·282	330·6	1·3	1
			8·695	339 6	1·4	4
η Cass.	0 43	+ 57 18	1891·059	190·8	4·7	4
Σ 60			2·245	194·6	4·8	4
			3·218	198·8	4·7	4
			4·084	202·3	4·9	5
			6·545	209·9	4·8	5
			8·795	214·6	4·5	5
			1900·787	222·7	5·0	10
36 Androm.	0 49	+ 23 4	1894·037	12·4	1·0	3
Σ 73			6·896	15·6	1·3	3
			1900·786	17·0	1·0	8
γ² Androm.	1 58	+ 41 51	1896·833	117·6	0·5	5
OΣ 38			8·742	115·4	0·5	13
			9·561	115·4	0·5	4
14 *i* Orionis	5 2	+ 8 22	1891·079	189 9	1·1	2
OΣ 98			4·136	186·5	0·9	7
			6·108	183·7	1·0	4
			8·885	182·1	0·9	3
			9·033	180·7	...	1
η Orionis ...	5 19	− 2 29	1891·082	84·2	1·2	2
			2·451	84·1	1·1	8
			4·128	84·4	1·0	8
			6·154	86·5	...	5
			8·953	86·2	...	1
			9·024	81·1	1·0	6
12 Lyncis, A.B.	6 37	+ 59 34	1891·075	122·3	1·8	2
Σ 948			2·745	123·3	1·6	3
			1900·199	120·3	2·0	3
Castor	7 28	+ 32 7	1890·358	229 8	5·5	6
Σ 1110			1·107	230·0	5·6	10
			2 209	229·7	6 0	11
			3·272	228·1	5 7	10
			4·082	233 1	5·8	3
			5 281	227·7	5·6	5
			8·944	226·0	5·5	5
			9 177	225 1	5·8	19
			1900·492	225 7	5 6	16

Name.	R.A.	Dec.	Date.	Position Angle.	Distance.	Nights.
	h m	° ′		°	″	
ζ Cancri, A.B. Σ 1196	8 6	+17 58	1891·131	33·0	1·1	5
			2·419	27·2	1 0	6
			3·220	28 2	1·0	6
			4·060	23·3	1·0	4
			5 287	21·5	1 0	4
			6·265	18·2	1·1	6
			9·086	11·1	1·0	5
			1900·295	5·8	1·0	4
ω Leonis Σ 1356	9 23	+9 30	1890·252	102·1	0·7	5
			1·239	103 6	0·6	14
			2·244	103·5	0·7	9
			3·283	104·3	0·7	15
			4·093	104·4	0·8	7
			6·143	109·5	0·8	9
			7·360	107·0	0·8	6
			9·178	111·1	0·9	14
			1900·268	111·2	0·9	12
γ Leonis Σ 1424	10 14	+20 21	1890·251	114·1	3·7	5
			1·174	113·6	3·8	5
			2·255	112·9	4·1	10
			3·220	114·7	3·5	6
			4·131	116·3	3·7	5
			6·213	116·8	3·6	13
			7·360	114·7	3·7	6
			8·945	117·0	...	1
			9·194	116·4	3·9	16
			1900·266	116·3	3·6	11
ξ Ursæ Majoris Σ 1523	11 13	+32 6	1890·249	211·8	1·7	4
			1·273	201 0	1·3	18
			2·296	192·7	1·8	15
			3·301	184·7	1·7	17
			4·205	180·9	1·8	14
			6·272	173·9	2·0	7
			9·234	152·9	2·1	9
			1900·300	151·0	2·1	15
ι Leonis Σ 1536	... 11 19	+11 5	1897·257	55·9	2·4	1
			1900·317	57·0	2·2	3

Name.	R.A.	Dec.	Date.	Position Angle.	Distance.	Nights.
	h m	° ′		°	″	
γ Virginis ...	12 37	− 0 54	1890·246	153·4	5·5	3
Σ 1670			1·304	153·1	5·6	11
			2·336	151·8	5·8	11
			3·291	151·6	5·7	16
			4·247	151·3	5·5	9
			5·261	331·8	5·6	3
			6·266	332·3	5·7	6
			7·363	331·4	5·7	8
			1900·271	331·1	5·6	10
35 Comæ Ber.	12 48	+ 21 48	1895·501	75·6	1·0	4
Σ 1687			1900·313	77·0	1·1	2
25 Canum V. ...	13 33	+ 36 48	1895·501	137·2	1·0	4
Σ 1768			8·517	132·2	1·0	1
			1900·313	129·9	1·2	2
ξ Boötis ...	14 47	+ 19 37	1893·381	236·9	3·1	5
Σ 1888			4·346	232·7	3·1	4
			5·500	227·0	3·2	4
			7·531	216·7	2·5	1
			8·657	210·5	2·4	9
η Cor. Bor. ...	15 19	+ 30 39	1891·312	216·8	...	10
Σ 1937			2·489	234·?	...	12
			3·499	240·1	0·5	11
			4·295	261·7	0·4	3
			5·589	285·1	0·4	4
			1897·520	328·3	0·5	14
			8·517	343·4	0·5	1
			1900·382	5·5	0·7	6
μ^2 Boötis ...	15 21	+ 37 43	1892·392	94·1	0·8	4
Σ 1938			3·674	85·4	0·6	1
			4·297	85·9	0·9	2
			7·535	79·3	0·9	4
			8·657	77·3	0·9	10
γ Cor. Bor. ...	15 39	+ 26 37	1891·313	120·2	0·5	11
Σ 1967			2·567	120·0	...	4
			3·630	119·5	0·6	9
			4·321	118·6	0·7	3
			5·419	117·4	0·5	1
			7·513	116·7	0·7	12
			8·517	115·2	0·7	1
			1900·415	116·1	...	3

Name.	R.A. h m	Dec. ° ′	Date.	Position Angle. °	Distance. ″	Nights.
σ Cor. Bor. ... Σ 2032	16 11	+34 7	1892·560	206·7	4·0	2
			3·455	209·6	4·2	1
			4·328	210·3	4·0	3
			5·500	211·9	4·4	4
			7·509	208·8	4·3	14
			8·517	209·6	4·1	1
			1900·350	212·7	4·4	7
λ Ophiuchi ... Σ 2055	16 26	+ 2 12	1891·359	47·3	1·4	1
			2·693	47·5	1·5	2
			3·463	48·8	1·4	10
			7·545	49·3	1·3	4
ζ Herculis ... Σ 2084	16 38	+31 47	1893·449	54·4	1·4	4
			5·631	29·8	0·9	2
			7·543	16·1	0·7	4
			8·625	352·4	0·5	6
			1900·420	263	...	2
70 Ophiuchi ... Σ 2272	18 0	+ 2 33	1892·686	317·8	2·3	2
			3·449	311·8	2·4	8
			5·597	296·6	2·2	1
			6·887	289·7	2·0	3
			7·550	283·4	2·0	6
ε Lyræ ... Σ 2382	18 41	+39 32	1900·522	12·5	3·4	9
5 Lyræ ... Σ 2383	18 41	+39 31	1900·522	130·5	2·5	9
δ Cygni ... Σ 2579	19 42	+44 53	1890·715	310·7	1·4	4
			2·705	308·3	1·5	8
			3·613	305·6	1·6	20
			5·714	307·2	1·6	3
			6·871	304·5	1·7	5
			7·622	303·7	1·7	9
			8·676	302·5	1·7	11
			1900·621	127·2	1·5	17
β Delphini ... β 151	20 33	+14 15	1892·758	330·4	0·4	3
			3·702	341·5	0·5	15
			6·857	355·3	0·7	7
			7·648	359·6	0·7	5
			8·711	2·4	0·8	20
			1900·655	8·0	0·7	23

Name.	R.A. h m	Dec. ° ′	Date.	Position Angle. °	Distance. ″	Nights.
λ Cygni ...	20 43	+36 8	1890·734	77·5	...	2
OΣ 413			2·706	73·4	...	2
			3·588	71·8	0·7	30
			5·714	73·8	0·6	3
			6·871	70·5	0·6	5
			7·564	69·1	0·6	5
			8·754	68·9	0·6	11
			1900·648	64·8	0·6	18
ε Equulei ...	20 54	+ 3 55	1896·842	284·6	0·7	3
Σ 2737			8·746	282·2	0·8	11
			1900·652	289·8	0·6	10
61 Cygni ...	21 2	+38 16	1895·597	124·0	20·3	1
Σ 2758			1900·857	124·9	22·0	7
δ Equulei ...	21 10	+ 9 37	1896·842	201·3?	...	3
OΣ 535			8·775	201·8	...	7
52 Pegasi ...	22 54	+11 12	1896·875	218·6	1·0	5
OΣ 483			7·663	219·2	1·0	3
			8·870	218·5	1·0	7
			1900·637	220·4	1·0	4
85 Pegasi ...	23 57	+26 34	1896·875	208·0	0·8	5
β 85			7·638	223·3	0·7	4
			8·816	234·4	0·7	8

Observations of Mars made at Mr. Edward Crossley's Observatory, Bermerside, Halifax, during the Opposition of 1900–1901. By Joseph Gledhill.

The following observations were made with the 9-inch photo-visual object-glass of the Cooke Equatorial. The powers used were 240 and 330. The observing conditions were almost invariably poor, and never really good.

1901 February 11, 9^h. The Kaiser Sea was about central, and formed a very striking feature; its extreme northern portion, where it is joined to the western end of Nasmyth Inlet, was not seen. Nasmyth Inlet and Lassell Sea were often but not steadily seen. The dark Knobel Sea lay near the eastern limb. The N. polar cap was of course a very prominent feature;

its surface and southern edge were carefully examined, but no markings or irregularities of outline were seen. The beautiful ruddy colour of Beer Continent was well seen, and it extended quite up to the eastern limb of the planet. On this and every other clear night the terminator was very frequently examined for protuberances or projections, but none were seen.

The details of the features about the S. pole were never well made out during this opposition.

No markings of any kind were ever seen in the central portions of Beer Continent or Secchi Continent.

February 13. Much motion. Saw all that was seen on the 11th and saw it much better. Delambre Sea lay close to the western limb. So also on February 14.

February 17. Clear night ; much motion ; definition as bad at midnight as at 9^h. The Kaiser Sea was about central at $11\frac{1}{2}^h$. The features seen were as above, with the exception of the beautiful warm colour of the Beer Continent, which was not seen. Main Sea was often looked for but never seen.

March 12. Much motion and flare ; planet examined at short intervals for some six hours. Knobel Sea dark and very prominent. The curved band of seas to the N. of the S. pole was also an easy feature, but details could not be seen. The S. end of Knobel Sea extended as far S. as the centre of the disc. The curved broad band of seas was greenish.

March 16. Fair definition to-night. At 8^h several features were pretty well seen, and so remained for hours. The broad band round the S. pole was darkest at its eastern end, *i.e.* De la Rue Ocean ; then, to the west, was the lighter Phillips Island. The region round the S. pole was nearly as bright as Beer Continent, which occupied the western half of the central part of the disc. Dawes Forked Bay and Burton Bay were dark as usual. The western limb was much brighter than the eastern ; the darkest portion of the limb was where De la Rue Ocean touched it. Knobel Sea lay to the east of the central meridian, and extended from near the N. polar cap to about the equator of the disc ; it has always, during this opposition, been one of the darkest features visible. To the west of this sea lay two streaks, slanting from N.W. to S.E., viz. a branch of Delambre Sea and Lassell Sea ; these were faint markings, the latter being the darker of the two. There was a faint red glow over Beer Continent.

March 21, 8^h. The western edge or limb very bright ; nearly as bright as the N. polar cap ; this brightness extended inwards to a distance equal to about one-fifth of the radius of the planet. The eastern limb was not nearly so bright. The Kaiser Sea was a fine object, and lay just west of the central meridian. The N. edge of Herschel II. Strait was dark ; the

region including Dawes Ocean was much brighter ; still brighter was Lockyer Land round the S. pole. The fine line joining the Kaiser Sea (its N. end) to the extreme western end of Nasmyth Inlet was seen occasionally. Nasmyth Inlet and a narrow band parallel to it to the north (an arm of Delambre Sea) were also seen. Beer Continent was of a uniform delicate red colour. At 10^h the Kaiser Sea lay far to the west of the central meridian, the dark N. edge of Herschel II. Strait (with the projecting bays), Nasmyth Inlet, Lassell Sea, the western part of Knobel Sea, and the arm of Delambre Sea were all well seen. No markings were seen on Beer Continent.

March 25, 8^h to 12^h. The features seen were about as on the 21st.

March 26, 9^h to 12^h. No warm colour E. or W. of the Kaiser Sea. The difference in brightness between the *p* and *f* limbs not at all marked as on the 21st.

March 28, $9\frac{1}{2}^h$. The Kaiser Sea was between the central meridian and the *f* limb; a dusky form lay close to *p* limb (Oudemans Sea ?), and on its eastern side there was a bright circular area (Fontana Land ?) ; the arm of Delambre Sea lay near (to N.W.) the N. end of the Kaiser Sea. A warm tint was spread over Herschel I. Continent. The *p* limb was bright ; the *f* limb less so.

March 31, $9\frac{1}{2}^h$. The *p* limb very bright ; the *f* limb less so. The Kaiser Sea lay near the *f* limb. Fontana Land, Oudemans Sea, Delambre Sea, seen as on 28th. The surface near the S. pole (Lockyer Land ?) was bright.

April 1, 8^h. The bright circular Fontana Land was near (a little to E.) the central meridian and the centre of the disc. Oudemans Sea was close to the west side of Fontana Land, and was a narrow marking. The *p* limb was very bright, and the brightness extended one-fifth of the radius inwards ; the *f* limb was not bright. The curved band extending from limb to limb to the N. of the S. pole was dark ; it was Maraldi Sea, &c.

April 10. Definition very bad ; bright *p* limb as on the 1st. No warm colour on the disc.

April 11, 8^h. Bad definition ; little clear sky. The *p* limb was very bright, and the brightness extended inwards as on the 1st. The darkest marking was at the N. edge of the broad curved band near S. pole, and close to the *p* limb. The only other markings seen were two faint streaks lying one near the centre of the disc and one between that and the N. pole. The curved broad band near the S. pole was faint.

April 13, 8^h. The small dark marking on the N. edge of the faint curved band of seas, &c., near the S. pole was on the inner edge of the very bright lune which lay along the *p* limb.

The two faint slanting markings, one passing through the centre of the disc (from N.W. to S.E.), and the other to the N. of it and parallel to it, were again seen as on the 11th. Secchi Continent probably occupied all the middle region of the disc; its eastern portion had less warm colour than the rest.

April 23, 9^h. Knobel Sea, dark, extended from the N. polar cap to the centre of the disc. Airy Sea lay to the east of it, and Lassell Sea to the west. The forked bays in Herschel II. Strait were also seen. No warm colour on the disc. The bright Rosse Land not seen. The bright yellow region near the S. pole was probably Jacob Land. The *p* limb was not very bright; it was very bright on the 20th and 21st. On the 24th the same features were seen as on the 23rd.

April 25, 8^h to 9^h. Knobel Sea was near the *f* limb, and the Kaiser Sea lay close to the *p* limb as a dark line. When the Kaiser Sea entered the very bright lune along the *p* limb it disappeared or became very faint indeed. There was a very slight warm (ruddy) colour over the central portion of the disc.

April 26, 8^h and 9^h. The Kaiser Sea lay near the *p* limb and along it; the darkest part of the sea could be seen when quite inside the bright lune, which lay along the *p* limb. Knobel Sea was near the *f* limb. The greenish band of seas and lands to the N. of the S. pole was the only other feature seen. Lockyer Land was seen as a bright yellow region surrounding the S. pole. Later, the forked bays seen, dark; Lassell Sea glimpsed now and then.

April 30, 8^h and 9^h. The Kaiser Sea near the central meridian; its faint and narrow northern portion was near the centre of the disc. This narrow portion was not seen to extend up to the western end of Nasmyth Inlet. Nasmyth Inlet and Lassell Sea not well seen. Delambre Sea and Lockyer Land seen. Flammarion Sea, dark, was seen quite up to the *p* limb. Dawes Ocean extended quite up to the *f* limb without any loss of distinctness or tone.

Lyrids, 1901 *April, observed at Cambridge.*

By J. C. W. Herschel.

1. I watched for the *Lyrid* meteors on April 12 and 17 to 22 to see whether I could trace any motion of the radiant, as suspected by Mr. Denning. I noted down 60 meteors, of which 40 came from the neighbourhood of *Lyra*. Of these I have rejected 6 as not well enough noted to use, and 2 as being too far from the radiant (see § 4). The remaining 32 distribute themselves among six centres, as tabulated below.

Mean Centre. α	δ	Ap. 12.	Ap. 17.	Ap. 18.	Ap. 19.	Ap. 20.	Ap. 21.	Ap. 22.	
I.	267·9	+28·1	...	...	...	...	1	2	...	= 3↓'s
II.	273·5	+27·7	...	...	...	1	2	1	...	= 4
III.	274·5	+33·6	...	...	...	1	1	1	3	= 6
IV.	278·0	+37·3	2	1	1	1	4	2	...	= 11
V.	278·1	+30·8	...	...	...	1	0	3	1	= 5
VI.	281·4	+32·5	...	...	...	...	1	2	...	= 3

Unfortunately none of these centres furnished enough meteors day by day to give a satisfactory daily determination of the radiant point ; the crossing point of two meteors is not enough to build evidence upon ; if motion exists, I have not enough data to show it (but see § 8). But, on the other hand, as all the tracks pass within one degree of their assigned radiants—and none have been omitted because they would not fit in—I can fairly treat the radiants as stationary, and on this assumption I find (without weights) the mean centres for the whole period, as given in the first column. The effect of zenith attraction would be less than $\frac{3}{4}°$.

2. With many lines crossing each other at all angles over a small space there are sure to be several points of concentration or apparent radiant points, and it is by no means easy to distinguish the real from the fortuitous. These *Lyrids*, for instance, show more apparent radiant points than the six I finally chose ; and it may be of interest if I describe in general terms my method of discrimination.

Perhaps one or two will be so strongly determined as to stand out obviously. Four meteors (or at the least three well noted ones) are necessary for this first step. Such are radiants III. and IV. Eliminating these by making a fresh tracing of the rest will probably eliminate also one or two fortuitous centres, and bring out more clearly weaker radiant points, indicated by short close meteors. Even two only may be enough as an *indication* for the second step. Radiant VI. was found in this way from the 7th and 8th meteors given in table, § 4. For the third step a single meteor is enough if it has been also observed elsewhere (radiant II.) ; and, finally, a radiant point given by a doubly observed meteor not seen at all by the observer can be taken as an indication (*e.g.* radiant I.). On going through this process for the first time it is very likely that, if any are left, they will seem to be stray single ones, and unassignable ; but by repeating the process several times, and reassigning meteors which pass through more than one radiant, and may belong to either, an arrangement will most probably be found to suit them all, with a fair probability of its being the right arrangement.

3. Five of the meteors seen here were observed elsewhere. Professor Herschel has found their real paths and radiant points, and the following particulars of their courses, with notes on two of the paths which were derived from triple observations, are taken from a list of sixteen such real path determinations of meteors seen this year on April 13–21, which he kindly supplied me with.

Date. April.	Statn.	Hour. G.M.T.	Mag.	Dur.	From α	From δ	To α	To δ	Description.	Heights (miles) at Beginning.	Heights (miles) at End.	Length m.	Speed m/s.	Theor. Speed.	Radiant. α	Radiant. δ
19*	S.	12 45½	>1	0·6	354°	+77°	30°	+73°	wh., no str.	77m over 6m	54m over	32	32	32¼	265° ± 1½°	+24°
	C.	12 46	1	1·0	212	+64	165	+64	orange, broad str. 1s.	N. of Ramsey, Hunts.	6m NNE. of Oundle, Notts.				19° S. of E.	
	M.	12 45	2	v. quick	258	+11	250	−3½							alt. 14½°	
20†	Ba.	10 25	2	...	298	+42	317	+43	...	57m over North	52m over do.	32	20	24½	220°	−21°
	C.	10 22	1	1·6	283½	+38½	296	+44	red or yellow	Sea, 2° 44′ E.	2° 20′ E.				32° S. of E.	
	M.	10 21	2	r. slow.	255	+7	261	+11½	...	53° 11′ N.	53° 33′ N.				alt. 14½°.	
20	C.	11 36 (used for radiant II.)	4	1·2	255	+47½	236	+57	wh., str. ½s dirn. well noted.	70m over 4m	49m over 2m	40	33	32	274°	+28°
	M.	11 35	1	r. quick	252	+1½	245	−7½		N.W. of Halesworth, Suffolk.	S. of Mildenhall, Suffolk.				6° N. of E. alt. 32°.	
21	C.	11 35	1	slow.	11	+57	24	+50	length & dirn. approx.	55m over 55m	37m over 48m	34	...	28	241°	+5°
	M.	11 34	2	quick.	300	+42	323	+45	...	E. of Saltburn, Yorks.	E. of Sunderland, Durham.				40 S. of E alt. 32½°.	
21	C.	11 45½	>1	1·5	210	+64	163	+61½ ±	broad red or yel. str. 1s.	69m over 2m.	56m over 3m.	25	17	33½	272°	+23°
	M.	11 44	3	v. quick	245	+1	240	−3½	...	N.W. of Ely, Camb.	S. of Holme, Hunts.				2° S. of E. alt. 32°.	

I also add, as found by Mr. Denning (used for radiant I.):

21	B.	10 9	=4	1·8	278½	+52	304	+70	slowish, str. 7s.	81m over 5m	51m over 3m	72	40	[30½]	268°	+30.
	M.	10 7	>1	quick.	200	+8	178	−4	str. 3–4 sec.	S.W. of Stanford, Rutland.	E. of Droitwich, Worcestershire.					

The observers and stations were :—

S. Professor Herschel, at Slough.
B. Mr. Denning, at Bristol.
M. Mr. Brook, at Meltham, near Huddersfield.
Ba. Mr. Holmes, at East Barnet.
C. Mr. Herschel, at Cambridge.

Notes.

* This meteor may have been a Lyrid from about 266° + 26°, the middle point on the Cambridge backward path, between the tolerably broadside intersections with it, about 3½° apart, of the two backward Slough and Meltham paths, at 265° + 27° and 267° + 24°. The latter two paths, directed in *Cepheus* and *Ophiuchus* almost oppositely away from Lyrid points on their path-lines at 266° + 30° and 271° + 30°, diverged with small inclination to each other in their crossing, from a point of concurrence several degrees south of all those places, at 263° + 19°, and in the narrow-based triangle with this acute-angled summit, a point as nearly as possible equidistant from its three sides was taken, at 265° + 24°, as given in the list for the real path's mean radiant-point. Should the Cambridge path's direction, however, as seems more reasonably probable, have been exactly right, the mean radiant, found as above from the two other paths' very broadside intersections with it, would be at about 266° + 26°, more nearly included in the Lyrid radiant area than the supposed point, as given in the table, at 265° + 24°.

† Although the three times of appearance differ rather largely (which recurred, however, similarly in the same lists in other accordance cases), these three path-descriptions all certainly referred to the same meteor, which, appearing in an unusual quarter, E. and N.E., in the Meltham and East Barnet lists, agreed perfectly in those lists in base-line direction and amount of displacement from the Cambridge path. The two best situated paths, at Cambridge and Meltham, to fix the radiant-point gave a backward intersection at 231° − 12°, near *β Libræ*, and of that point's exact correctness there could be no doubt had the East Barnet path conformed to it. Mr. Holmes' observations there were a first experience in meteor mapping, and the mapped track was so far astray in its direction as, when lengthened backwards, to cross the Cambridge path itself about one-third way from its commencement, and, carried further back, it was so nearly parallel to the mapped course at Meltham as not to intersect that track's back-prolongation at any point above the southern sensible horizon. As there is a marked region of active, though not yet well located radiant-points during the April meteor period on the south-western side of *Libra*, although the above intersection point at 231° − 12° is not far from the positions, 234½° − 13½° and 235° − 15°, given in Mr. Denning's General Catalogue and in his list of co-Lyrid meteor-showers (in *Astr. Nachr.* No. 3513) of the notable April shower of *η Librids*, the rather dubiously small angle at which the chief pair of paths converge to it, and the third path's apparent alignment to some more southward radiant-focus, made the presumed radiant-point at 220° − 21° adopted in the list, 14° along the medial direction axis of the Cambridge and Meltham path-lines, backwards from their simple intersection, commend itself for trial and assumption, as several recorded meteor-paths in this and last year's Lyrid watches appeared to diverge very consistently and distinctly from a productive focal centre there, near *ι Libræ*. For a similar reason of a suspected meteor-shower near *β Libræ*, a real path of this meteor, differing but little from the present one, was extracted from its three recorded paths by Mr. Denning, with an adopted radiant-point at 227° − 16°, on the same line of mean direction, but only 7° behind the place of intersection of the two chief backward path-lines noted at Cambridge and Meltham. Those two path-lines both pass backwards within one degree of that assumed position, and within two degrees of the radiant-point position near *ι Libræ* used in this real path's construction.

4. In *Monthly Notices*, 1899 March, vol. lix., p. 333, Mr. Denning says: "In endeavouring to find whether motion occurs in a radiant, only such meteors should be utilised as are well observed, and situated near their radiants. If observers set themselves to accumulate observations of this kind, we should in a few years have the means of disposing of some vexed questions in this branch of observational astronomy."

(I was glad to light upon this opinion, for nearly all my tracks *end* within 45° of *Vega*, this being as large a field as I have found that I can properly watch, and I had to let some half-dozen *Lyrids* go on this account, besides the two mentioned in § 1). Accordingly I give a list of my best observed meteors. I give the places accurately as plotted at the time, but the decimal is of course uncertain. The last column gives the perpendicular on the track from the assigned radiant point as a measure of the accuracy.

Date. April. 1901	Hour. G.M.T. h m	Mag.	Dur. Sec..	From α δ	To α δ	Description.	Radi. Assigned. α δ	p, in arc. ′
12	*11 15	2	...	285·4 + 32·3	290 5 + 27·4	blue.	278·0 + 37·3	17
	*11 30	2	...	288·5 + 37·5	295·4 + 37·0	,,	,, ,,	24
17	12 48	> 1	·4	289·6 + 31·1	293·6 + 28·8	bright blue.	,, ,,	17
18	11 44	1	·2	281·1 + 33 7	282·3 + 31·7	,,	,, ,,	11
19	11 12	1·5	·9	305·2 + 33·7	312·6 + 31·7	red, on horiz.	,, ,,	31
	14 43	2	1·0 ±	255·6 + 35·4	239·8 + 35·7	wh., sl. str.	278·1 + 30·8	13
20	14 15	1	1·1	280·0 + 37·5	279·4 + 40·0	,, ,,	281·4 + 32·5	10
21	10 52	1	·7	270·7 + 36·1	265·4 + 37·0	white.	,, ,,	10
	12 49	1	·6	253·2 + 29·6	247·2 + 26·8	bright blue.	278·0 + 37·3	5

5. For a first determination of the radiant point without weights I have used a simplification of Schiaparelli's graphic method, for which I am indebted to Mr. H. C. Plummer. Schiaparelli drops perpendiculars on all the tracks from three corners of a square round the assumed radiant. Mr. Plummer uses two points on the axes (and the origin implicitly). Indeed, it is evident that two conditions ought to be enough to find the two coordinates required:

Let $x \cos \alpha + y \sin \alpha - p_0 = 0$ be the equation to a meteor; then

$$\left.\begin{aligned} x\Sigma \cos^2 \alpha + y\Sigma \sin \alpha \cos \alpha - \Sigma p_0 \cos \alpha &= 0 \\ x\Sigma \sin \alpha \cos \alpha + y\Sigma \sin^2 \alpha - \Sigma p_0 \sin \alpha &= 0 \end{aligned}\right\}$$

give the radiant. Let $(x,y)'$ and $(x,y)''$ be the coordinates of the feet of the perpendiculars on each track from $(0,\lambda)$ and $(\lambda,0)$, where λ is *any* convenient length, and putting

$$\sigma_1 = \Sigma x', \ \sigma_2 = \Sigma x'' \qquad \tau_1 = \Sigma y', \ \tau_2 = \Sigma y'' \qquad n = \text{no. of } \downarrow\text{'s}$$

* Observed at Littlemore, near Oxford, long. 1° 13′ W. lat. 51° 43′ N.

the equations are easily transformed into

$$x(\tau_1-\tau_2)+y(\sigma_2-\sigma_1) \qquad =\lambda(\sigma_2+\tau_1-n\lambda)$$
$$x(\sigma_1-\sigma_2+n\lambda)+y(\tau_1-\tau_2-n\lambda)=\lambda(\sigma_1-\tau_2)$$

It is often convenient to take one λ, or both, negative: this is equivalent to turning round the axes; it is less confusing to turn round the equations; the appropriate changes are readily found.

6. I have made use of Mr. Cookson's expression (as corrected by Mr. Plummer) (*Monthly Notices*, 1901 January and March), to find the weights. I have left over the probable errors until I have accumulated more observations. The term depending on p_r (the perpendicular from the graphically deduced radiant point, § 5) I have found to be *always* negligible, so that the expression for the weight, g, reduces to

$$g=\frac{D^2}{P}$$

simply,* with the curious result that the weight is independent of the accuracy as to direction—the only element used in the graphic method—and I should define the criterion of a meteor's right to be included in the determination of a radiant to be, "when the p term begins to be sensible the meteor's right begins to be doubtful." The weight, then, is a rough measure rather of the *usefulness* of including the meteor than of its *right*. Weighting the equations would alter my radiant points less than half a degree.

7. In a list which Mr. Denning has kindly sent me of Lyrids and others seen by him at Bristol between April 13–24 he has grouped together twenty meteors as Lyrids without assigning a definite radiant point. They form, in fact, just such a group as I start with, and discussing them in the same way as I have mine, I find that the group readily splits up into five radiant points, three of which confirm mine :—

Bristol.		Cambridge.	
α	δ	α	δ
267	+30	268	+28
268·5	+32·5		
270	+33		
274	+27	273·5	+28
273·5	+33·5	274·5	+33·5

* D is the length of the meteor-track

$$P=l^2(\lambda^2+l+D^2)+l+\overline{D^2}(\lambda^2+l^2)+p_r{}^2(l^2+\overline{l+D^2})$$

in which λ, the radius of projection, or scale of the map, can be put = unity, and l is the distance of the beginning point of the meteor-track from the foot of the perpendicular p_r. This is a slight transformation of Mr. Cookson's formula, which I have found more easy to use in practice. P, as a whole, is identically the same, and $p_r{}^2(l^2+\overline{l+D^2})$ is the negligible term.

The two distant meteors I have rejected, and one which I have assigned to Radiant III. may have come from 270+33. Mr. Denning's meteors were mostly on the 21st; there are not enough on other days to prove motion of the radiant.

8. Radiant IV. is probably quite a distinct stream from the true Lyrids (see Denning's Catalogue). Its characteristic bright blue meteors (§ 4) like Roman candles are in strong contrast to the red broad-streaked Lyrids. As to the others (though one must be cautious in drawing deductions from so few meteors), they show no tendency to sequence of appearance in time, *i.e.* to one radiant in motion, but rather contemporaneous activity; and, further, they occupy an area about 10° by 5°, whose length lies much in the same direction as the suspected motion. May not an area of such a shape give the impression of a radiant in motion, and yet really contain stationary radiant points with perhaps variable activity? The shape also suggests some spreading action *in process*, such as Professor Turner suggests as an explanation of stationary radiants; but if so, I do not see how it can be due to the Earth, as it is not parallel to the ecliptic, but to the Milky Way.

Corrections to reduce the Revised Madras Catalogue of Stars for 1835·0 *to the Fundamental Catalogue of Auwers* (*Pub. Ast. Gesell.* xiv. and xvii.). By A. M. W. Downing, M.A., D.Sc., F.R.S.

In the recently issued edition of Taylor's Madras Catalogue for 1835 the clock errors were determined from the right ascensions of standard stars in Auwers' Fundamental Catalogue (*Pub. Ast. Gesell.* Nos. xiv. and xvii.), whilst the index errors were determined from the declinations of the same catalogue corrected so as to represent Auwers' Mean System (*Ast. Nach.* No. 1536).

It may be of interest to give here the corrections applicable to the places of the Revised Madras Catalogue to reduce them to the system of Auwers given in *Pub. Ast. Gesell.* Nos. xiv. and xvii.

It should be noted that the mean $\Delta\alpha$ is $+^{s}\cdot015$ from 402 stars, and the mean $\Delta\delta$ is $-0''\cdot44$ from 297 stars.

The limits of declination for $\Delta\alpha$ and $\Delta\delta$ are determined by the limits adopted for the stars used for clock error and index error respectively.

TABLE I.

Corrections in order of Declination.

Limits of Dec.	$\Delta\alpha$	Number of Stars.	$\Delta\delta$	Number of Stars
	s		"	
+70° to +60°	+·101	26		
60 „ 55	+·075	19		
55 „ 50	+·038	12	+0·37	14
50 „ 45	+·061	18	+0·47	18
45 „ 40	+·092	19	+0·14	21
40 „ 35	+·056	27	−0·20	28
35 „ 30	+·029	19	−0·13	21
30 „ 25	+·019	24	−0·08	26
25 „ 20	+·019	28	−0·77	24
20 „ 15	+·015	26	−0·65	26
15 „ 10	−·030	26	−1·08	26
10 „ 5	−·022	28	−0·64	29
+ 5 „ 0	·000	24	−0·96	23
0 „ − 5	−·017	18	−0·61	19
− 5 „ 10	−·019	18	−0·90	22
10 „ 15	−·028	21		
15 „ 20	−·032	20		
−20 „ −25	−·043	29		

TABLE II.

Corrections in order of Right Ascension.

Limits of R.A.	$\Delta\alpha$	Number of Stars.	$\Delta\delta$	Number of Stars.
h h	s		"	
0− 1	+·023	17	−0·49	11
1− 2	+·009	21	−0·84	14
2− 3	+·037	18	−0·40	15
3− 4	+·007	22	−0·68	16
4− 5	−·017	18	−0·58	15
5− 6	+·019	27	−0·45	24
6− 7	+·016	14	−0·70	8
7− 8	−·014	13	−0·20	11
8− 9	+·021	12	−0·24	8
9−10	−·011	14	−0·63	9
10−11	+·042	18	−0·25	7
11−12	−·006	18	−0·69	13
12−13	+·035	11	−0·42	5
13−14	+·022	11	−0·36	6

Limits of R.A. h h	$\Delta\alpha$ s	Number of Stars.	$\Delta\delta$ "	Number of Stars.
14–15	−·011	15	+0·51	14
15–16	+·007	16	−0·19	18
16–17	+·034	16	−0·40	12
17–18	−·028	13	−0·58	12
18–19	−·006	15	−0·37	11
19–20	+·051	17	−0·33	17
20–21	+·050	23	−0·73	15
21–22	+·034	18	−0·43	12
22–23	+·012	21	−0·60	13
23–24	+·026	14	−0·47	11

TABLE III.

Reduction Table.

Dec. °	$\Delta\alpha$ s	$\Delta\delta$ "	R.A. h	$\Delta\alpha$ s	$\Delta\delta$ "
+65	+·101		0	+·009	−·04
60	+·084		1	+·001	−·22
55	+·057		2	+·008	−·18
50	+·050	+·42	3	+·007	−·10
45	+·077	+·31	4	−·020	−·19
40	+·074	−·03	5	−·014	−·07
35	+·043	−·17	6	+·002	−·14
30	+·024	−·11	7	−·014	−·01
25	+·019	−·43	8	−·012	+·22
20	+·017	−·71	9	−·010	·00
15	−·008	−·87	10	·000	·00
10	−·026	−·86	11	+·003	−·03
+5	−·011	−·80	12	·000	−·12
0	−·009	−·79	13	+·014	+·05
−5	−·018	−·76	14	−·010	+·52
10	−·024		15	−·017	+·60
15	−·030		16	+·006	+·14
20	−·036		17	−·012	−·05
−25	−·043		18	−·032	−·03
			19	+·008	+·09
			20	+·035	−·08
			21	+·027	−·14
			22	+·008	−·08
			23	+·004	−·09
			24	+·009	−·04

The limits of declination for $\Delta\alpha$ and $\Delta\delta$ are determined by the limits adopted for the stars used for clock error and index error respectively.

Errata in the Revised Madras Catalogue of Stars for 1835·0.

By A. M. W. Downing, M.A., D.Sc., F.R.S.

Herr Ristenpart has had the kindness to send me a list of discrepancies between the recorded places of stars in the Revised Madras Catalogue compared with other authorities. An examination of these has enabled me to draw up the following list of errata. I feel greatly indebted to Herr Ristenpart for the trouble he has taken in the matter.

No. 7. Mean Dec. 1835·0, *for* $-3°\ 28'\ 38''{\cdot}67$ *read* $-3°\ 28'\ 44''{\cdot}67$

No. 48. Mean Dec. 1835·0, *for* $+0°\ 46'\ 22''{\cdot}71$ *read* $+0°\ 46'\ 15''{\cdot}99$.

No. 320. Mean R.A. 1835·0, *for* $0^h\ 55^m\ 15^s{\cdot}32$ *read* $0^h\ 56^m\ 15^s{\cdot}32$. Precession in R.A., *for* $+3^s{\cdot}106$ *read* $+3^s{\cdot}107$. Mean Dec. 1835·0, *for* $+6°\ 9'\ 48''{\cdot}60$ *read* $+6°\ 9'\ 48''{\cdot}64$. Precession in Dec., *for* $+19''{\cdot}478$ *read* $+19''{\cdot}457$.

No. 701. Star's name, *for* 16 Arietis *read* Taylor 705. Delete the references to Bradley and Piazzi.

No. 2928. Mean R.A., 1835·0, *for* $7^h\ 8^m\ 57^s{\cdot}85$ *read* $7^h\ 7^m\ 57^s{\cdot}85$. Mean Dec. 1835·0, *for* $+9°\ 44'\ 59''{\cdot}85$ *read* $+9°\ 44'\ 59''{\cdot}74$. Precession in Dec., *for* $-5''{\cdot}945$ *read* $-5''{\cdot}862$.

No. 3609. Taylor's No., *for* 3626 *read* $\left\{\begin{matrix}3625\\3626\end{matrix}\right\}$.

No. 3965. Mean R.A. 1835·0, *for* $8^h\ 59^m\ 51^s{\cdot}66$ *read* $8^h\ 59^m\ 51^s{\cdot}69$. Mean Date in R.A., *for* 31·98 *read* 32·11. No. of Obs. in R.A., *for* 7 *read* 12.

No. 3966. Mean R.A. 1835·0, *for* $8^h\ 59^m\ 51^s{\cdot}74$ *read* $9^h\ 0^m$ ... Mean Date in R.A., *for* 32·29 *read* ... No. of Obs. in R.A., *for* 5 *read* .... Precession in R.A., *for* $+3^s{\cdot}467$ *read* $+3^s{\cdot}465$. Mean Dec. 1835·0, *for* $+22°\ 39'\ 42''{\cdot}29$ *read* $+22°\ 39'\ 42''{\cdot}12$. Precession in Dec., *for* $-14''{\cdot}173$ *read* $-14''{\cdot}236$.

No. 5965. Mean Dec. 1835·0, *for* $-2°\ 29'\ 47''{\cdot}17$ *read* $-2°\ 28'\ 47''{\cdot}17$.

No. 5966. Mean Dec. 1835·0, *for* $+31°\ 39'\ 37''{\cdot}45$ *read* $+31°\ 40'\ 37''{\cdot}45$.

No. 6040. Star's name, *for* Brisbane 4355 *read* Brisbane 4344. Mean R.A. 1835·0, *for* $13^h\ 2^m\ 9^s{\cdot}11$ *read* $13^h\ 1^m\ 9^s{\cdot}15$. Precession in R.A., *for* $+3^s{\cdot}661$ *read* $+3^s{\cdot}651$. Mean Dec. 1835·0, *for* $-58°\ 41'\ 47''{\cdot}99$ *read* $-58°\ 41'\ 47''{\cdot}91$. Precession in Dec., *for* $-19''{\cdot}326$ *read* $-19''{\cdot}348$.

No. 6413. Mean Dec. 1835·0, *for* $+22°\ 5'\ 16''{\cdot}61$ *read* $+22°\ 5'\ 10''{\cdot}45$.

No. 6718. Mean R.A. 1835·0, *for* $14^h\ 17^m\ 9^s{\cdot}62$ *read* $14^h\ 18^m\ 9^s{\cdot}62$. Mean Dec 1835·0, *for* $-0°\ 20'\ 15''{\cdot}12$ *read* $-0°\ 20'\ 15''{\cdot}20$. Precession in Dec., *for* $-16''{\cdot}572$ *read* $-16''{\cdot}522$.

No. 7233. Mean R.A. 1835·0, *for* $15^h\ 24^m\ 42^s{\cdot}92$ *read* $15^h\ 24^m\ 45^s{\cdot}37$. Precession in Dec., *for* $-12''{\cdot}574$ *read* $-12''{\cdot}572$.

No. 10179. Magnitude, *for* 9 *read* 6.

Nautical Almanac Office:
1901 *June* 22.

Errata in MONTHLY NOTICES, vol. lxi. p. 128.

Line 12, equation (15) should read *as follows*:

$$p = (2 - e^2 \pm 2\sqrt{1 - e^2})/e^2 = (1 \pm \sqrt{1 - e^2})^2/e^2$$

and equation (16) should read *as follows*:

$$\mathrm{OS} = e(1 + p) = 2\sqrt{p} = (2 \pm 2\sqrt{1 - e^2})/e.$$

Add also these words:

"The lower sign is to be taken when AB < OA, as in figs. 4 and 6; and the upper sign when AB > OA, in which case the lower half of the ellipse is described first."

Same page, lines 22 and 23:

For the ellipse thus becomes a parabola, and S is at an infinite distance,

Read the ellipse thus becomes a straight line, and S is at one extremity.

[I am indebted to Dr. Otto Knopf for these corrections.—H. H. T.]